THURSDAY, AUGUST 6, 1891.

A PHYSICIST ON COLOUR-VISION.

Colour-Measurement and Mixture. By Captain Abney. (London: The Society for the Promotion of Christian Knowledge, 1891).

THIS interesting little book extends over only 200 pages, but is full of careful and important observations. It is, in fact, a summary of the results arrived at by the author during his careful and laborious investigation of the properties of the spectrum. It forms one of the "Romance of Science" series published by the S.P.C.K., a series intended "to show that science has for the masses as great an interest as, and more edification than, the romances of the day." Now, though the earlier portion of this book could be understood by anyone, we venture to think that the second half is for the most part so technical that the full meaning and value could only be appreciated by those who are more or less conversant with the methods of experimenting on colour. To those who are familiar to even a slight extent with the technicalities of colour experiments, the characteristic of the book is its extreme lucidity. We are carried on from point to point, until, when we look back on the closed book, we find we have travelled over the greater number of the problems of colour-vision almost without effort. It is a book which will not appeal to the masses, but should be read by every physiologist and physicist interested in colour-vision.

There is yet another reason for the interest which attaches to this work, necessitating a fuller notice than if it were simply a popular disquisition on colour. It is the record of a careful series of experiments by an eminent physicist, firmly convinced of the truth of the Young-Helmholtz theory of colour-vision. The voluminous work of Hering and his pupils is not once mentioned throughout the whole book, although König's later publications receive due notice. In fact, if space permitted, we cannot imagine a book more calculated to form the basis of a fruitful discussion on the merits of the rival theories than that now before us. For both the problems of colour-vision, and their solution according to the Young-Helmholtz theory, are definitely and clearly stated.

The book opens with a description of the methods used to obtain a spectrum, and a consideration of its properties with especial reference to the ultra-red and ultra-violet rays. The apparatus used by the author to investigate the three fundamental properties of colour—hue, luminosity, and purity—are described in detail. Absorption and interference are then touched upon in their relation to colour, and experiments are given to show that the colour of a body is due to its refusal to transmit or reflect certain rays of the spectrum. This is followed by an interesting chapter on scattered light, with especial reference to atmospheric effects, and a pretty lecture-room experiment is described to show that the change in the colour of the sun when on the horizon is produced by small particles in the air.

The author then passes on to consider the second property of colour—luminosity; and the luminosity of the spectral colours is measured as follows. The light from a certain portion of the spectrum passes through a slit which cuts off the remainder of the spectrum. A portion of the same white beam which was decomposed by the prisms is reflected on to the same screen as the monochromatic beam, and an upright rod is interposed. This rod throws two shadows, whose intensity is compared after the manner of a photometer. The luminosity of the whole reflected beam is greater than that of the coloured beam, and a rotating diaphragm, with variable sectors, is therefore interposed in its course. By altering the size of the sectors, the intensity of the white light is diminished, until the luminosity of the shadow it casts is equal to that cast by the monochromatic beam. The luminosity is then read off in terms of the segment of the circular diaphragm which remains open when the luminosity of the two shadows is equalized. The luminosity of all the principal points in the spectrum is measured on this plan. Subsequently the luminosity of a combination of red and green is shown to be equal to the sum of the luminosities of the same red and green determined separately. Three colours, A, B, and C, are chosen, which, when combined, make white of a certain intensity, W; and the author shows that if the luminosity of the combined light $A + B$ be subtracted from the luminosity of the white light, W, the remainder exactly equals the luminosity of the third factor, C.

A curve of luminosity can be constructed in this manner for the whole spectrum, and its maximum is found to lie on the yellow side of the D line. A similar luminosity curve is given for an observer who was what is ordinarily called red-blind. On this curve the red end of the spectrum is shortened, and the maximum luminosity falls nearer the green than on the curve constructed for a person with normal colour-vision. These facts are explained as follows. To the red-blind observer red is invisible, and therefore the luminosity of red is abolished; the luminosity of yellow, which is composed of red and green, is also diminished, and thus the maximum of the curve moves towards the green.

This question of luminosity is intimately associated with the theory of the value of white in the system of colour. The author discusses later on in the book the abolition of colour by white light, and examines the extent to which white light can be added to a colour without being perceived. He finds that both depend on the luminosity of the colour, and formulates the law that "the extinction of every colour is effected by white light that is 75 times brighter than the colour." Again, he finds that a large proportion of white light can be mixed with yellow without being perceived, whilst a very small proportion of white added to blue is at once apparent.

An attempt is made to explain these facts on the Young-Helmholtz theory; but the work done by Hillebrand,[1] under Hering's guidance, makes the explanation offered very improbable. Hillebrand used an apparatus in which one half of the field could be illuminated by a monochromatic spectral colour, whilst the other half was illuminated by white light. The observer shielded one eye from the light for a considerable time, so that it was ultimately brought into a condition of complete rest. Now if he looked at a field filled with monochromatic

[1] "Ueber die specifische Helligkeit der Farben," *Sitzb. d. k. Akad. d. Wissenschaft. in Wien*, February 1889.

light of moderate intensity with the rested eye, it appeared to him colourless; and by suitable adjustment he could make an absolute match between the half of the field illuminated by monochromatic light and the other half illuminated by white light from the same source. Thus, as the whole spectrum appeared colourless, he was able to construct a curve of luminosity for the spectrum by matching it with the white light in the other half of the field. The maximum of this curve lay in the green. A glance with the unshaded eye at once brought the colour into view, although the field was unaltered. But as soon as the colour came into view, he noticed that the luminosity of the coloured half no longer matched that of the colourless half of the field. If yellow or red were the colour chosen, the luminosity of the coloured half of the field appeared to exceed that of the colourless half, whilst if green or blue were selected the exact opposite was observed. Moreover, as soon as the colours of the spectrum were appreciated, the maximum luminosity shifted into the yellow, and the curve he then obtained closely resembled that constructed by Captain Abney and other observers. Thus we must conclude that every part of the spectrum is capable of exciting the sensation of white apart from its specific colour, and that the maximum sensation is produced by a certain point in the green. As soon, however, as the colour becomes apparent, this sensation of white is either increased or decreased by the specific luminosity of the colour. The luminosity of the spectrum, as determined by Captain Abney, is the algebraic sum of two factors. Firstly, the power which every part of the spectrum possesses of exciting the sensation of white; and secondly, the specific luminosity of the colour sensation itself, which is a positive quantity on the red and yellow side and a negative quantity in the blue and green.

If this explanation for the difference in the two curves be correct, a person who was completely deficient in colour-sense would construct a luminosity curve for the spectrum differing considerably in the position of its maximum from that given by Captain Abney in his book. The curve obtained by König[1] from a man to whom yellow, blue, green, and red were invisible, to whom the whole spectrum appeared in varying shades of white, shows that this is the case. The maximum luminosity lies in the green, over the line *b*. A comparison of this curve with that given by Hillebrand for the normal eye at rest reveals their almost absolute identity. The existence of this form of colour-blindness can only be explained with extreme difficulty on the Young-Helmholtz theory; whereas Hering's hypothesis, that white and black form a colour pair analogous to red and green, yellow and blue, not only renders the existence of such a condition probable, but also easily explains Hillebrand's results.

The author passes on to show that white can be produced from the mixture of three spectral colours, and ultimately defines a primary colour as one which cannot be formed by the mixture of any other colours. The three primary colours he selects are red, green, and violet; for yellow is formed by a mixture of red and green, blue by a mixture of green and violet. But he warns us from assuming that the three primary colour sensations "are of necessity the same sensations as are given by the three primary colours" (p. 138). On p. 150, red (between C and the lithium line), violet (close to G), are selected as furnishing two primary sensations, whilst "all three fundamental sensations" are excited by the green, except at a point where the green is mixed with white only.

Now, to say that spectral green excites the sensations of red and violet seems to us radically false. For when speaking of sensations we leave the realm of physics, and the sole test of the sensations excited by a portion of the spectrum is the colour which we perceive when light from that part impinges on the retina. No one who examines spectral green will say that it gives him the sensation of red or violet, but rather that the greater part of spectral green appears to be mixed with either yellow or blue. Again, a primary sensation must be one which gives us the sensation of one colour only. Now every eye sees in violet both blue and red. Thus, whether violet be a primary colour from the physical point of view, physiologically speaking it is anything but a primary sensation.

Though violet fails to answer the test of a primary colour sensation, a point can be found both in the yellow and the blue of the spectrum, from which the sensation of one colour only is obtained. But throughout the book we find repeated mention of the formation of yellow by the mixture of spectral red and spectral green. How can this be reconciled with the acceptance of yellow as a primary sensation?

To most eyes, the red of the spectrum yields to a greater or less extent the secondary sensation of yellow. Take such a red, and gradually add minute quantities of spectral blue. The yellow will gradually disappear, and a red will be produced, which yields the sensation of red only, untinged with either yellow or blue. Take a spectral green, which is also slightly yellow, and treat it in the same way. If we now mix the absolutely pure red with the absolutely pure green, white is produced, not yellow. And now we can understand why spectral red and spectral green can be made to form yellow. For both the red and the green, which, when mixed, form yellow, when separate give the secondary sensation of yellow in addition to that of their principal colour. Thus, when mixed, the pure red annihilates the pure green, and yellow only remains. Measured by this standard, the primary colour sensations fall into two groups, in which each colour is complementary to the other. Firstly, red and green, from which all secondary sensations of yellow and blue are absent; and secondly, yellow and blue, which do not give the secondary sensations of either red or green.

Colour-blindness is brought in to support the Young-Helmholtz theory, but the author has obviously not had the opportunity of investigating many cases of this affection. He speaks of green-blindness, in which the sensations of red and violet are present, but not that of green; and of red-blindness, in which the sensations of green and violet are present, but not that of red; and gives measurements to show that in the latter class of cases the spectrum is shortened.

[1] "Die Grundempfindungen u. ihre Intensitäts-Vertheilung im Spectrum," *Sitzb. d. k. preus. Akad. d. Wissenschaft. zu Berlin*, xxxix, 1886. Hering has since shown, by investigating a similar case of total colour-blindness, how closely the curve of luminosity agrees with that given by Hillebrand. The account of this interesting case has not yet been published.

Now, Hering[1] has particularly investigated this portion of the subject, and explains the existence of two forms of colour-blindness as follows. He finds that persons with a normal colour-vision can be divided into two groups. The one class perceive yellow, the other blue, with exceptional ease, probably owing to a difference in the pigmentation of the media of the eye. The difference between the two groups is best seen with spectral green; for a green can be found which appears at the same time yellow-green to the one, blue-green to the other. To an observer with strong yellow vision, almost the whole of spectral red appears to be tinged with yellow, whilst a member of the second group, whose strong sense of blue prevents his seeing the yellow, pronounces the greater part to be pure red. Thus, the pure red and the pure blue are radically different colours for the two groups. Now, it is found that the pure red and the pure green formed for an observer with a strong sense of yellow appear grey to one who is what is called green-blind; whilst, on the other hand, the pure red or the pure green of the observer with a strong blue sense appears colourless to one who is red-blind. A red which is invisible to one who is "red-blind" is evidently coloured to a patient who is green blind, and he speaks of the colour he sees as red. But if a minute proportion of blue is added, the red gradually becomes purer until it becomes free from yellow to those of us who have a strong yellow sense. As the red becomes purer, our green-blind patient complains that the "red" is fading, and when finally the red is quite pure he matches the colour he sees with a grey, and says that the colour has gone. Thus, there is no fundamental difference between the red- and the green-blind. Neither group can perceive red or green. The only difference between them is one which we find amongst normal-sighted persons—namely, a different visual acuity for yellow and blue. The "red" of the green-blind is in reality the secondary sensation of yellow yielded by almost all the reds in nature, differing from the ordinary yellow in its limited power of exciting white. This peculiar yellow he has learnt to associate with what others around him call red, and he only betrays his affliction when all yellow is eliminated from the colour he calls red. Thus, a consideration of colour-blindness again leads us to throw red and green, blue and yellow, together into two groups as primary colour sensations.

Simultaneous contrast is touched on very superficially, and successive contrast is scarcely mentioned, yet the author again grasps at the three-colour theory to explain the few phenomena he mentions. Yet it is notorious that the Young-Helmholtz theory fails to afford any adequate explanation of the phenomena of contrast. It was by an ingenious contrast experiment that Hering produced such a striking confirmation of his views before the Physiological Congress at Basle, and placed the three-colour theory in a dilemma from which its ablest exponents have not yet succeeded in extracting it.

In conclusion, the book before us is an admirable summary of a valuable series of experiments. We can scarcely imagine that it will appeal to the public in general. But it should be read by those who are interested in the phenomenon of colour-vision, and the fact that the author frankly accepts the three-colour theory and ignores the work of Hering does not, in our opinion, detract from its value. For the book thus becomes an admirable statement of the strongest portion of the physical theory of colour by one of the ablest of English physicists.

H. H.

[1] "Zur Erklärung d. Farbenblindheit" (Prag, 1880); "Ueber Individuelle Verschiedenheiten des Farbensinnes" (Prag, 1885); "Eine Vorrichtung z. diagnose d. Farbenblindheit," "Ueber d. Erklärung d. periphären Farbenblindheit," "Einseitige Störungen d. Farbensinnes," *Archiv f. Ophthalmologie*, xxxvi.

POSITIVE SCIENCE AND THE SPHINX.

Riddles of the Sphinx. A Study in the Philosophy of Evolution. By a Troglodyte. (London: Swan Sonnenschein, 1891.)

THESE be old old riddles that the Sphinx propounds, and the Troglodyte attempts to guess, in the volume before us; none other, indeed, than the What, Whence, and Whither of man and of the world. There have been other guesses in the past, there will be other guesses while time lasts; each guesser thinks his own guess nearer the true answer than any other; his neighbours mostly smile, unless his guess chances to be something like their own; and the Sphinx looks on with stony stare, imperturbable, giving no hints.

So soon as man, as man, looked out upon the world, and began dimly to realize the first personal pronoun, the nascent reason, or, if the phrase be preferred, intellectual faculty, demanded, for the first time in the history of the development of consciousness, an explanation. Man, then as now the chief centre of interest to man, must thenceforward not only live and act, but must seek to explain his life, and his activity. Yesterday the tribe-chief went forth a living man, feared by all: to-day his body is brought back, helpless, lifeless, and a hog spurns it with his snout. How account for this? How explain this change? Something there was about the man yesterday which made him totally different from the mere mass of clay that to-day already needs hustling out of sight. That something, call it soul, spirit, energy, life, what you will, has departed. Whither has it gone?

This question, eminently natural, almost inevitable, opened the way for reason's first blunder to enter and to become a fruitful mother of children. Reason, in the exercise of the new-born analytic faculty, distinguished between the mere body and the informing something through which it was a living body; between the material substance and the spirit-energy which was associated with that substance during life. But reason also jumped to the conclusion that what were distinguishable in thought were also capable of separate existence in fact. The matter remains in the corpse, but the something, the spirit-energy, has escaped, to lead a distinct and independent existence. In justification of this conclusion the phenomena of dreams were no doubt adduced as evidence. While the chief's body was lying stark and stiff, his true self, his spirit-energy, appeared by night to more than one of his chosen followers. Thus the dream seemed to support the false conclusion of the nascent reason, which had not yet learnt to distinguish without dividing.

It has cost positive science much labour, and not a few hard blows, to establish, by detailed work in physical science, biology, neurology, and psychology, the ille-

gitimacy of this conclusion. Now we distinguish further, but no longer divide. We distinguish between the material substance of the body and the energy of molecular motion during life; and, further, between the molecular motion of the grey matter of the cerebral hemispheres and the concomitant manifestation of consciousness. But although consciousness is distinguishable from molecular energy (and the distinction is absolute), it is not, so far as positive science can say, divisible therefrom. No physicist holds that the special modes of energy—we mean the particular groupings and interactions of energy—which characterize the functioning of a man's brain, escape from the molecules at death, and henceforward persist divorced from matter. We cannot, however, add that no psychologist holds an analogous doctrine concerning consciousness. But we contend that no psychologist is justified *on positive grounds* in holding such a view. That something called soul or spirit escapes from a man's body at death, and henceforward persists, divorced alike from matter and energy, is a view to which positive science as such gives no support. It is held by those who hold it on quite other grounds. The conclusion to which positive science points (and we include among positive sciences psychology, which deals with consciousness as existent) is that consciousness, though distinguishable from energy, is known only in association with certain forms of energy in organic tissues.

But this is a conclusion which is ignored by the Troglodyte. He professes to give us a "philosophy of evolution" which he himself describes as "the first perhaps which accepts without reserve the data of modern science." His theory of a Transcendental Ego; his suggestion that "matter is an admirably calculated machinery for regulating, limiting, and restraining the consciousness which it encases"; his conception of a graduated immortality, from that of an amœba up to that of man; his attempted rehabilitation of the view that force-atoms are monads "endowed with something like intelligence, and thus enabled to keep their positions with respect to one another"; all this, and much besides, seems to us completely off the lines of modern scientific advance.

But it may be said that such conceptions, though unnecessary for positive science, may be necessary for a philosophy which endeavours to go beyond and get behind science. In reply to this we can only say that we regard such conceptions as not only unnecessary to positive science, but unwarrantable intrusions into her domain. They form part of a different scheme of thought. The muddling together of positive and metaphysical conceptions is provocative of nothing but confusion and bad temper.

The introductory chapters of his first book, in which the author attempts to hound on positive science from agnosticism, through universal scepticism, to a gloomy pessimism, seem to us laboured and inconclusive, though there are incidental positions here and elsewhere with which we are in complete accord. With dogmatic Agnosticism and the Cult of the Unknowable (capital letters indispensable) we have but little sympathy. But this is no necessary part of the attitude of positive science, which seems to us briefly as follows. In the first place its followers take their start from the measurable and verifiable base-line of perceptual experience, from the ordinary facts of daily observation; and they utterly refuse, at this stage of the inquiry, to listen to the metaphysicians who hoot from their cloud-land, "But you haven't yet proved the existence of matter, or explained how it is possible to perceive or know anything at all." Starting, then, from the base-line of perceptual experience, they analyze phenomena, digging down by wise abstraction and the ignoring of unessentials, to deeper and deeper concepts, until they arrive at those universal abstracts which cannot be got rid of in thought without reaching nonentity. Happy they who in this procedure escape the analyst's fallacy—the supposition that the results of abstraction have a fuller reality than the phenomena with which they started. The analyst needs often to be reminded that the perceptual rose, with its delicate scent, its rich colour, its soft petals, is certainly not less real than the vibrating molecules which remain to his thought when, as physicist, he has stripped it of all its own peculiar charms.

Thus positive science in its deepest analysis brings us down to matter, and energy, and consciousness. If a number of metaphysical questions are intruded at all sorts of stages during this process, the result will be such confusion as the Troglodyte unconsciously exemplifies in his chapter on scepticism, a chapter in which some stress is laid on, and some capital made out of, the false psychological conclusion that conceptions cannot be derived from experience. Should the author ever come to grasp that the law of psychogenesis is one and indivisible, and sweeps through perceptual and conceptual processes alike, he will have to rewrite much of the "Riddles of the Sphinx." But, as he himself tells us, "the minds of most men are fortresses impenetrable to the most obvious fact, unless it can open up a correspondence with some of the prejudices within."

When positive science has dug down to basal conceptions, then, and not till then, in logical order (but, of course, far earlier in historical order) arises the question, "But how does it all come about? What is the origin and meaning of it?" We quite agree with the Troglodyte that this question *must* arise in the mind of every man in so far as he is a thinking man. The question, "How does it all come about?" however, presents two faces. It may mean, "How can we explain the fact of *knowing?*" And the solution of this problem is, we agree with Mr. Shadworth Hodgson in maintaining, the true business of philosophy. But even supposing that philosophy explains in some sense the process of knowing, there still remains the question in its further aspect, "But how does it all come about?" To this question, positive science as such answers, or should answer, humbly, and with no parade of capital letters, "I do not know."

And is that the end of the matter? So far as positive science at present goes, Yes. But man, the questioner, still remains; and Reason, true to her first impulse, still demands an explanation. Of the explanation afforded by revelation this is not the place to speak. But, quite apart from the fact of revelation, the explanation said to be revealed still stands as a product of the human mind. And he is a bold man, if not a foolish, who, having regard to the past history of human thought on the question, lightly sets aside the conception of a *Causa causarum* to whom we may attribute *symbolically* all the higher

attributes of man; not because personality, wisdom, love (the symbols we employ), can truly describe or define that which passes man's comprehension, but because being man we can no other. Man alone in the organic world is capable of ideals, and for generations the name of God has stood for man's central ideal of power and perfection. And it seems to us that the sum and substance of positive criticism as applied to man's conceptions of that which admittedly lies beyond the reach of positive science comes to this: "You must frankly acknowledge and confess that such conceptions are symbolic and ideal." But if symbolic and ideal we must expect the symbolism to be variable in different ages, among different peoples, and even in different individuals. Hence (apart from revelation) the only indefensible attitude is that of inelastic dogmatism, positive or negative.

In conclusion, we may say that the "Riddles of the Sphinx" are in this work treated with considerable, though frequently misguided, power. The conception of evolution as a tendency towards an ideal of perfect individuals in a perfect society is good, and is in parts well worked out. That many will be found to acquiesce in the author's solutions of the old problems of life we think exceedingly doubtful. Nor do we think that the solutions will prove of lasting value. It is futile to attempt to preserve the new wine of positive science in the old bottles of prescientific metaphysics. The new wine must be preserved in new bottles. In other words, a new metaphysics must be and is being elaborated, in special relation to the newer aspects of scientific thought.

C. LL. M.

ANALYTICAL METHODS OF AGRICULTURAL CHEMISTS.

Proceedings of the Association of Official Agricultural Chemists, 1890. (Washington: United States Department of Agriculture.)

THIS is a Report of the Seventh Annual Convention of the Association, under the Presidentship of Mr. M. A. Scovell, and with Mr. H. W. Wiley as Secretary. The objects of the Association are to secure uniformity and accuracy of methods, results, and modes of statements of analyses of manures, soils, cattle foods, dairy products, and other materials connected with agricultural industry; and to afford opportunity for the discussion of matters of interest to agricultural chemists. In the words of a past President, it aims at laying "a foundation so solid, that every Court in this land must respect its conclusions, and every analytical chemist, whether he lives in this country or elsewhere, must be forced either to practice or admit the advantages and correctness of our system of analyses." A study of the programme and of the proceedings shows that the objects have been most carefully and conscientiously kept in view, and that all the working members have been most thoroughly imbued with the spirit of the Association.

The reports submitted for the consideration of the meetings, all drawn up by experts, and incorporating the work of many members, were as follows: on the determination of nitrogen; on analysis of dairy products; on analysis of potash; on analysis of cattle foods; on analysis of sugar; on analysis of phosphoric acid; on analysis of fermented liquors; and a report of a Committee on foods and feeding-stuffs.

As an example: for the report on the determination of nitrogen in manures, three samples, containing nitrogen in different states of combination, were prepared, and sent to the members for analysis by various official methods. Twenty-two reported the results obtained by Kjeldahl's method on one sample, the same number the results of Kjeldahl's method modified for nitrates on two samples, and a less number gave results by the Ruffle method, the soda-lime method, and Dumas's method on one or more of the samples. The whole of the results are collated, with the remarks of the analysts thereon, so that data are obtained for testing the accuracy of the methods under various conditions, and eliminating personal factors. Various suggestions for the improvement or simplification of the processes are made and discussed, and some of them recommended for systematic trial during next year. Similar good work is done for the other Committees.

The remarks of the Committee on ways and means for securing more thorough chemical study of foods and feeding-stuffs, are particularly worthy of attention, pointing out, as they do, the deficiencies in present methods of analysis, and the absolute necessity of more exact methods and more accurate study of the proximate principles contained in foods, and of their physiological value. As a contribution towards this knowledge, Mr. W. E. Stone sends a paper on the occurrences and estimation of the pentaglucoses in feeding-stuffs, in which he shows that bodies yielding furfurol, and therefore presumably pentaglucoses, are present in grass, straw, linseed meal, and a great many other feeding-stuffs. Among the points which are noticed, and which should be known to all analysts, is the fact that cotton-seed meal, often used in mixed manures in the Southern States, is completely soluble in nitric acid with a little hydrochloric acid, but that the solution does not yield all its phosphoric acid to ammonium molybdate.

Should such a Bill as that introduced by Mr. Channing, for the better prevention of the adulteration of manures and feeding-stuffs in this country, ever become law—and the Government has promised to take up the matter—the formation of such an Association of Official Agricultural and Analytical Chemists in this country would be almost a necessity, and it seems that the Institute of Chemistry is the proper body to arrange the organization of such an Association.

GEOLOGICAL RAMBLES ROUND ABOUT LONDON.

Hand-book of the London Geological Field Class. By Prof. H. G. Seeley, F.R.S. (London: G. Philip and Son, 1891.)

THIS little book is a record of excursions similar in some respects to those collected in the volume of Geological Excursions which was noticed in these columns on June 18 (p. 149). But there are points of difference. This hand-book deals with a more limited area, being practically restricted to the south-east of England; it has a purpose more definitely educational. The latter may

be described in a few sentences extracted from the preface:—

"This Society exists to teach the elements of Physical Geography and Geology direct from Nature without preliminary study from books. . . . The field work has been led up to by short courses of winter lectures given in London, designed to connect together the observations to be made in the succeeding summer, and to connect the geology of the district to be examined with that of other areas."

The excursions are described in the notes written by students in the field; the lectures are reported (from shorthand notes) by Mr. White, one of the class. As regards the former, Prof. Seeley states that "students have been free to report what they saw and what they heard, and they have severally written in their own ways both as to length and language used." The lectures also "were not constructed with a view to being reported, nor were the reports written out with a view to being printed." Prof. Seeley has, however, "read the proof to remove serious inaccuracies." The lectures need no apology, for they are excellent examples of that clear and suggestive method of teaching of which Prof. Seeley is a master. The reports of the excursions also acquire a certain freshness as recording the impressions of novices, and may on that account be even more helpful to beginners than if they had been written by more experienced observers. One or two inaccuracies, however, appear to have escaped the Professor's watchful eye. Is not the statement on p. 18, relating to the presence of *Paludina* and *Unio* in such Wealden Limestones as the Petworth Marble, a little misleading? for it implies that the latter genus is common in these deposits, which, we believe, is not the case. A sentence on p. 29 suggests that "enormous pressure" is requisite to convert a sandstone into a quartzite. Very probably this would be the result, but there are not a few quartzites which show no signs of having been specially subjected to pressure. Also, it is hardly correct to call Lydian stone an altered sandstone. Again, more than once it is intimated that gneiss and crystalline schists occur in Belgium. This, if the terms be used in their ordinary sense, is incorrect; and even the porphyroids and amphibolites, and the abnormal rocks of the Bastogne district, the vague descriptions of which may have given rise to this misconception, are of extremely limited extent. But these are very trifling blemishes, which can be readily removed in a second edition. The book will be of great use to all students, living in or about London, in helping them to use their eyes; and most of all because, to quote Prof. Seeley's words, "It here and there touches upon problems which are not usually presented to beginners." But, as he rightly urges, these problems—namely, the application of stratigraphy to the elucidation of the physical geology of past epochs—"should never be absent from the mind of anyone who considers geological facts in the field."

T. G. B.

OUR BOOK SHELF.

Katalog der Bibliothek der Deutschen Seewarte zu Hamburg. (Hamburg, 1890.)

VARIOUS notices have from time to time appeared in NATURE relating to the German Naval Observatory at Hamburg, describing the building, its equipment of instruments, and the important work which is carried on there chiefly in the interests of the German Imperial and mercantile navies.

As this institution is possessed of a library containing some 10,660 works, it has for some time past been a matter of urgent necessity that an accurate and well-considered form of Catalogue should be printed and published. The required book was completed last year, and is now available.

This Catalogue shows that the library contains a large proportion of works either directly of a naval character, or bearing upon naval matters, whilst several other branches of science are fairly represented.

As might be expected, meteorology holds the first place of importance, and amongst the 2769 works on this subject are a large proportion of Dove's writings. Indeed, it seems worthy of note that Dove's library, which occupied him many years in collecting, may now be found at the German Naval Observatory. Turning to the division of the Catalogue on physics, 1617 works will be found; on magnetism and electricity, 974; whilst other subjects, such as navigation, hydrography, and construction of ships are well cared for.

Although the books and papers mentioned in this Catalogue are generally printed in the language adopted by their authors, a translation into German of several works of interest is also placed side by side with the original.

In conclusion it may be remarked that although there is nothing specially new in the arrangement of this book, it is well worthy of the time and energy which have evidently been spent in bringing the work to its present state.

Scientific Results of the Second Yarkand Mission; based upon the Collections and Notes of the late Ferdinand Stoliczka, Ph.D.—Coleoptera. By H. W. Bates, F.R.S., J. B. Baly, D. Sharp, F.R.S., O. Janson, and F. Bates. Pp. 1-79 and 2 Plates. (Calcutta: Published by order of the Government of India, 1890.)

THIS, the twelfth part issued, all but one of which deal with zoology, contains an enumeration of 207 species of Coleoptera. These species belong to the following families:—Cicindelidæ (4), Carabidæ (60), Longicornia (5), Phytophaga (25), Haliplidæ (1), Dytiscidæ (8), Gyrinidæ (1), Hydrophilidæ (3), Staphylinidæ (9), Scarabæidæ (38), Cetoniidæ (3), and Heteromera (50). Diagnoses or descriptions of all the new genera and species were published more than ten years ago, and the only additional information contained in this part is a list of species, in addition to, in some cases, fuller descriptions of the novelties. In the portions contributed by Mr. H. W. Bates and Dr. Baly, both of whom, however, give some particulars regarding geographical distribution, the references to the published diagnoses are given; but in Dr. Sharp's and Mr. F. Bates's contributions, many of the genera and species are mentioned as new, though diagnoses of the whole of them were published in 1878 or 1879—the former in the Journal of the Asiatic Society of Bengal, xlvii. Part 2 (1878), the latter in *Cistula Entomologica*, ii. (February 1879). The two plates include 44 figures—Carabidæ (17), Longicornia (5), and Heteromera (22). On the cover, and also on p. 37, the name "Hydrophilidæ" is misprinted "Hydroptilidæ." The Hydroptilidæ do not belong to the order Coleoptera at all, but to the Neuroptera! It is to be regretted that a delay, the cause of which is not explained, of more than ten years, has occurred in the publication of the "Part" dealing with the Coleoptera, as works of this kind upon the beetle fauna of little-known districts are always of the highest value, more particularly in the matter of geographical distribution. No systematic work upon the Coleopterous fauna of India has

yet been published, and even a fragment like the present, containing a list of the species of a neighbouring region, is a welcome addition to our knowledge. Four other "Parts" have been issued on the Insecta—the "Neuroptera" and "Hymenoptera" (both in 1878), and the "Lepidoptera" and "Rhynchota" (both in 1879); the last Part of the whole series being the "Araneidea" (1885).

Popular Astronomy. By Sir George B. Airy, K.C.B. Seventh Edition. Revised by H. H. Turner, M.A., B.Sc. (London: Macmillan and Co., 1891.)

ALTHOUGH our astronomical knowledge has been enormously extended since the lectures forming the basis of this well-known book were delivered (1848), Mr. Turner has not found it necessary to make any very considerable revision, for the reason that the advances have been chiefly on the chemical and physical sides. Still, in the lapse of time, methods of observation have been improved, and accounts of these find a place in Mr. Turner's notes. Among these are short descriptions of the chronograph and the new "electrical controls" for the driving-clocks of equatorials. One of the most noteworthy points brought out in the new edition, however, is the modern estimate of the value of observations of the transit of Venus as a means of determining the solar parallax. It was formerly supposed that this would be one of the best methods, but the difficulties encountered in 1874 and 1882 prevented observations of the necessary degree of accuracy; and now most astronomers are of opinion that this method can never give more than an approximation to the truth. Numerous minor additions have also been judiciously made.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

Force and Determinism.

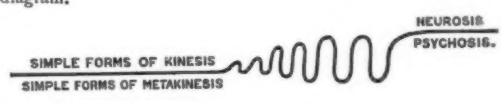
"THE relation between force, which is a mechanical thing, and will or life, or whatever it is, which is a psychological thing"—a relation which, as Dr. Lodge rightly says, "demands investigation"—presents itself to some of us as follows.

When a stimulus received by an organism gives rise to a response, however particular to the individual respondent, there are (1) a number of complex but determinate molecular changes in the organic tissues; and (2), accompanying some of these changes, certain psychological states. Are these psychological states produced by the molecular changes? or are the molecular changes produced or in any way guided by the psychological states? Neither the one nor the other. The molecular changes and the psychological states are different aspects of the same occurrences. In other words, they are distinguishable (and the distinction is absolute), but not divisible.

"The energy displayed by a gang of navvies is not theirs, but their victuals'; they simply direct it." In physiological language it is the outcome of the proper functioning of their cerebral control-centres. Now we believe that, although we can at present by no means adequately explain them, all the molecular occurrences within the organism, forming, as we believe they do, an orderly and determinate sequence between stimulus and response, whether they involve force or energy, are of such a nature as to be explicable in physical and physiological terms. The fact that certain phases of the sequence have also a subjective or psychological aspect does not, it is held, justify us in changing our point of view, and ignoring the distinction between the psychology and the physiology of the process.

Now to say that mind, or will, or consciousness directs the organic energy along a definite path we regard as incorrect, because it ignores a distinction which we hold to be valid and valuable, and conducive to clear thinking on these difficult subjects. But we have no such objection to the statement that the energy is guided by molecular forces which have for their subjective aspect certain states of consciousness. To unscientific folk this may sound mere quibbling; but to physicists, who have done so much to teach us the vital importance of accurate language for clear thinking, we look for support in drawing this distinction, unless the distinction can be shown to be either invalid or useless.

This distinction between force, energy, and the physical series (what I have elsewhere spoken of as *kinesis*) on the one hand, and thought, consciousness, and the psychical series (what I have elsewhere spoken of as *metakinesis*) on the other hand, we hold to be absolute; while at the same time we hold that consciousness is indivisible from particular (neural) modes of kinesis. And this distinction we hold to be especially valuable when questions of the origin and development of consciousness are under consideration. This may, perhaps, best be expressed by a diagram.



Now, looked at from above, this wriggle is supposed to represent the development, from simple forms of molecular transactions, of that complex form of kinesis which we call neurosis. From this point of view, all is force and energy or kinesis, and can become nothing else. Looked at from below, we have the development of consciousness. From what? We must not say from lower forms of energy or kinesis, because that involves jumping across the line, or, in other words, ignoring the distinction. From what, then? From those lower forms of "something-which-is-not-yet-consciousness-but-which-may-develop-into-consciousness," for which I have ventured to coin the term metakinesis.

I have elsewhere endeavoured to show that this view is not open to the objection that, since the kinetic sequence is a continuous and determinate one, consciousness is merely a by-product, and that an unconscious Darwin might have written and influenced the conduct of unconscious Englishmen. For consciousness, though it is distinguishable from, is, according to the hypothesis, no less inseparable from, certain complex modes of the kinetic process. As the world is constituted, such supposed kineses, separated from their metakinetic aspect, would not be the same kineses but something altogether different. In other words, it is with certain molecular transactions which have also a conscious aspect that, in the world of living beings of which we have practical knowledge, we have to deal.

It is essential that physicists and psychologists should work hand in hand. Both are endeavouring to explain the phenomena on positive lines. And if there is anything in the views that I have briefly sketched in the preceding paragraphs which runs counter to the conclusions of physics, it must go by the board, and give place to a more widely-consistent conclusion, to which physics, speaking with the voice of authority in its own special province, can give a cordial assent. C. LLOYD MORGAN.

I AM afraid that, as Prof. Lodge has accepted my "middle paragraph" so easily, he has failed to appreciate its point. For, if that paragraph is correct, the Professor's assertion, "Force is certainly necessary to direct the motion of matter," is only a truism, similar to the important geometrical theorem, "In any right-angled triangle, one angle is equal to 90°." On the other hand, Dr. Croll's assertion, to the effect that guidance is effected by "determinism," and *not* by force, is a contradiction in terms. For, by definition, that which changes motion *is* force. If, therefore, Prof. Lodge's assertion has any real meaning, he must have some independent definition of "force," and I should very much like to know what that is.

Again, Prof. Lodge in no way answers "the crux in my last paragraph." Prof. Lloyd Morgan implies in his last letter that, in the case of the sun altering the direction of motion of the earth, no energy is expended. This is, of course, only approximately true; and even in the case of his twirling his stick round his finger and thumb, as the stick is elastic, its forces of cohesion in reality do some small amount of work. It is indeed true that, if two particles were once connected by an *absolutely* inextensible string, the cohesion of the string would do no work. But what I pointed out was that, in order to bring such

a string into action, it would be necessary to wait till two particles were moving on paths with a common normal—an occurrence which must be infinitely rare. When Prof. Lodge says "an infinite mass can absorb any amount of momentum, without receiving a trace of energy, &c.," he forgets that the term "infinite" is only relative, "an infinite mass" being one whose change of velocity (or kinetic energy) consequent on a given change of momentum is negligible *for the purpose in hand.* It would not, I imagine, suit Prof. Lodge's purpose to suppose psychic forces might do a *little* work, so long as it was only a *very little?*

May I remind him of the old paradox, "What would happen if an irresistible force were brought to bear against an immovable post?" EDWARD T. DIXON.

12 Barkston Mansions, South Kensington, July 24.

THE discussion on this topic has gained in clearness by Prof. Lodge's conceding that "the same question—What determines the direction of the transfer of energy?—may doubtless be asked in connection with inanimate activity; . . . but in neither case do I know the answer."

Perhaps some more precision may be attained by expressing the question in other words.

The principle of conservation of energy reigns over the *quantitative* relations of all processes in nature, but it does not give any explanation of the *qualitative* changes of those processes. These changes and their conditions must in every case be found out by special experience. But, nevertheless, they are, in every accessible case, found to be subjected to fixed laws. A given substance undergoes evaporation or chemical transformation—dependent on or necessarily bound up with changes of heat into energy of molecular motion, or into chemical energy, or *vice versâ*—at a distinct degree of temperature, or under distinct conditions of electrical action. Inexplicable as these transformations of quality or form of energy remain for us, there is nothing undetermined in them, neither have we any right to such a supposition for the qualitative changes going on in plants and animals—their quantitative relations being likewise governed by the principle of conservation of energy.

But there is another phase of the question. Some unknown material changes in the brain are connected with phenomena of consciousness. Nothing can be more fallacious than to consider consciousness as a form of energy, and to suppose it in a relation of equivalence to such forms. How it is, that what to our physical conception, or outer sense, are processes in the brain (which, as such, may be more clearly understood in future), are, at the same time, to our psychical conception, or inner sense, phenomena of consciousness, or acts of will, is a question beyond the domain of physical science, and capable of elucidation only by transcendental philosophy. Whoever wishes for more light here, must study the "Kritik der reinen Vernunft," especially the chapters "Von den Paralogismen" and "Die Antinomien."

Schopenhauer, and others after him, have considered our power of will, or our conscious directing of motion, as the key for all qualitative processes in nature, these being considered as, in their essence, acts of will. But this is cutting the knot by means of a metaphysical assumption. D. WETTERHAN.

Freiburg, Badenia, July 27.

IN reading over the remarks of Dr. Lodge and Prof. Morgan upon Dr. Croll's views as to the direction of force, it appears to me that both have missed the point. Dr. Croll did not mean that a force at right angles to another does no work, but simply alters the direction. His view is that the change of direction is not caused by a force. Dr. Lodge says it is, although he acknowledges that the second force does no work. Further, Dr. Croll says, with regard to the first force, that its direction is quite apart from the force. The force cannot direct itself. This is the crucial point before we get to a second force or to a right angle. I fully acknowledge the importance of Dr. Lodge's principle, but it is not simply the indorsement of Dr. Croll's idea.

Prof. Morgan thinks Dr. Croll's view no argument in favour of theism. It does not prove that mind can or does affect matter. Perhaps it does not directly prove this, but, within its range, it seems to me an effective reply to mechanical atheism. We see direction, and if this does not come from force it must come from some other source. We know of no other source but mind. To talk of mind affecting matter denies the essence of mind by which it is distinct from matter, and makes it a mechanical *ab extrâ.* But try to banish it and it will come in somewhere. "Tamen usque recurret."

Dr. Croll's position seems to me to affect the first law of motion. Uniform motion in a straight line is in no way connected essentially with force, if his view is correct.

Dr. Lodge's principle appears to affect the second law of motion, and also the doctrine of impact and transference of force.

Further, it affects gravity. Gravity is always at right angles to the first law of motion, and therefore gravity is not a force; for that can not be a force which never exercises force.

T. TRAVERS SHERLOCK.

Congregational Church, Smethwick, July 25.

Technical Education for Farmers, Farriers, and Engine-Drivers.

KNOWING that you take very great interest in the various questions relating to technical education, I may give you a few particulars of an experiment which the Devon County Agricultural Society recently made at its Exmouth meeting. Being desirous of giving farmers, farriers, and those generally interested in the welfare of horses, some information on the scientific principles which underlie a proper performance of the duties of the farrier, and the correct form and mode of attachment of horses' shoes; and also of giving farmers and engine-drivers some practical and scientific instructions on the working and care of steam-engines, the Society approached the County Council with a view to a grant in aid of their object. The proposal was very warmly taken up by Mr. Lethbridge and other gentlemen who are well known for their active interest in education and other matters important to the welfare of the county, and a grant was obtained.

The Society secured the services of Prof. F. Smith, head of the Army Veterinary School, Aldershot, and of Mr. W. Worby Beaumont, and by these gentlemen lectures were given on each of the three days of the Society's meeting at Exmouth. The weather was very unfavourable on two days, but notwithstanding this the attendance at the lectures was large, and on the second and third days was larger than was expected, and was fully up to the accommodation provided. The audiences were remarkably attentive and appreciative, and in every respect the experiment proved successful. Many who were sceptical before the lectures of their value to working men, became convinced that not only is it possible to give working men information which is useful in an important degree in their daily work, but that the men are themselves quick to appreciate its value. I may mention that on one of the days nearly two hundred shoeing-smiths and a large number of farmers attended the horse-shoeing lectures, and on one day seventy-eight engine-drivers entered for the lecture on the steam-engine, and there were also in attendance a large number of working and gentlemen farmers.

Totnes, July 29. JOHN L. WINTER.

THE ERUPTION OF VESUVIUS OF JUNE 7, 1891.

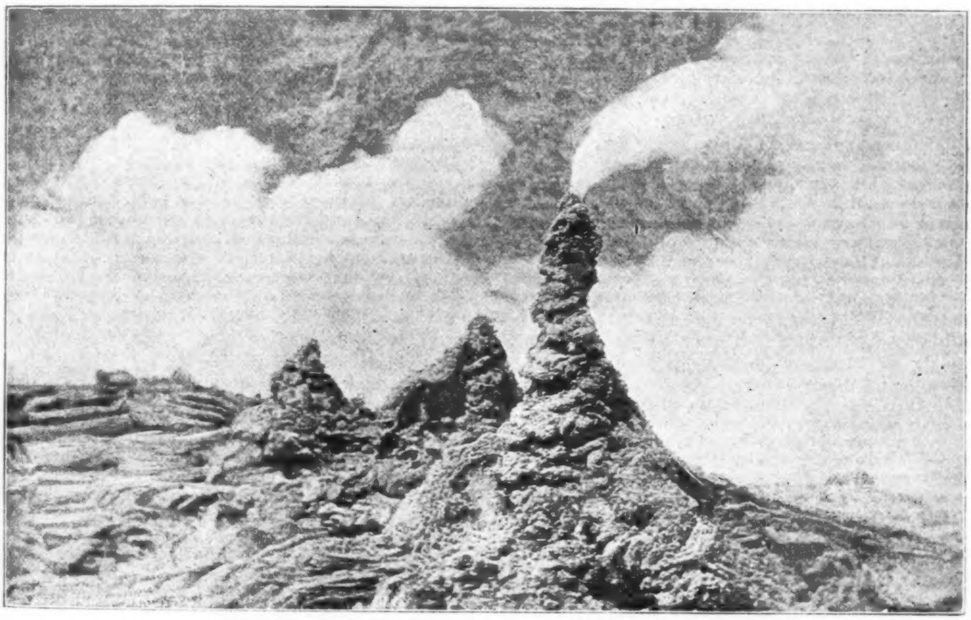
THE suggestion that I published in several newspapers has been fully confirmed—namely, that the second alternative type of eruptive character would be pursued by the volcano. Now for a period of over a month lava has continued to dribble forth, activity has returned to the central vent, and no great changes have occurred.

The throat of the volcano commenced to be cleared on June 9, the vapour forcing its way up from the crater bottom through the choke of loose materials, and rose above as a column carrying with it much dust; at the same time the powerful vapour blast issuing from the upper extremity of the lateral rift, of which mention is made in my first letter, soon stopped. Each day I was kept informed of the state of the volcano by the kindness of Messrs. Ferber and Treiber, the director and engineer respectively of the Vesuvian Railway.

On June 15 I considered it right to again visit the

mountain, and had the good fortune to be accompanied by Messrs. H. Elliot, A. Green, Linden, Newstead, and Treiber, several of whom are excellent photographers, so that with two of my own cameras we were able to make an extensive pictorial record of some very unique formations.

At the point of issue of the lava, at the junction of the foot of the great Vesuvian cone and the Atrio del Cavallo, the first lava had cooled sufficiently to walk over it, but beneath our feet could still be seen in a few holes the flowing lava. At the foot of the great cone, and extending for half way across the Atrio along the radius of the eruptive rent, as if this had continued so far, were a series of driblet cone fumaroles. We counted seven complete and well-formed examples, besides numerous abortive ones. Most were giving out intensely heated vapour, which was liberated from the lava flowing beneath, and which soon carbonized a piece of wood placed in it. Around the lips of the upper opening, hæmatite with fused chlorides of potash, soda, iron, copper, &c., were being condensed from the vapour, and trickling down the outer surface of the fumarole, consolidated as curious vari-coloured stalactites of very deliquescent nature.



The lava had first flowed towards the escarpment of Monte Somma in a fan-like manner, so that the eastern extremity reached that great natural section just beneath the Punta del Nasone. Still following the natural inclination of the ground, it turned to the west, and on June 15 was opposite dyke 16 (as marked on my large geological map just published, and on the dykes themselves), advancing at a very slow rate.

The lava is a vitreous and coarse-grained rock, especially in regard to the included leucite crystals, whilst the surface is, with one exceptional tongue, of the corded or "pahoehoe" type. This is due to the magma being one that has been simmering since January in the chimney of the volcano, so that most of its dissolved H_2O has been boiled off, and so allowing it to cool without the formation of scoria from the vapour that otherwise would escape after its exit. Leucite I have also demonstrated to be formed while the magma is simmering under low pressure with free escape for vapour in the upper part of the volcanic chimney.[1]

At the summit of the great cone the crumbling in of the edges was constantly going on, but the upper extremity of the lateral rift at the foot of the cone of eruption and at the summit of the great Vesuvian cone had nearly ceased to give forth vapour. Along the line of rent on the mountain side no fumaroles or other signs of activity were visible except quite at the foot, where those commence of which I have spoken.

Up till June 26 there was a struggle to clear the upper part of the volcanic chimney of the impeding materials, which were constantly being added to by the slips from the crater's edge; but on that evening a dull red glow was visible in the crater bottom, showing that a fairly clear passage had been temporarily made for the continuous escape of vapour, and also that the lava was at no very great depth from the summit of the volcano. This of course indicates that the lateral opening was insufficient to drain off much of the lava which occupies the chimney above the level of the lateral outlet. Had such evacuation really taken place, the eruption would have assumed enormous proportions, from the actual amount of lava above the tap, but more from frothing up of that below that level in consequence of the relief of pressure that in that case would occur. Of course, during all these days the ejection of dust with the smoke occurred, giving the latter that peculiar dark grey colour. Further destruction of the crater edge took place, so as to partly block the outlet, and it was not till our next visit that it again cleared.

On June 30 I again visited the crater, in company of my friend Mr. A. Green. All the summit of the great cone

[1] See H. J. J.-L., "Geol. M. Somma and Vesuvius," Q.J.G.S., vol. xl.; and "Relationship of the Structure of Igneous Rocks to the Conditions of their Formation," Scient. Proceed. R. Dublin Soc., vol. v., N.S.

was covered by a thick coating of dust and sand, upon the surface of which were the usual white and yellowish-green chloride crusts seen on such occasions, so rich in copper as to plate with that metal the iron nails of our boots. The crater had considerably enlarged, the edges were in an extremely unstable state, with often considerable strips marked off by cracks parallel to the free edge, so that, with a slight push by a stick, it was possible to detach large masses of the materials which form the sides of the crater in the recent cone of eruption. So dangerous were the edges, that it was but in two places that my experience indicated as being safe to approach and look over, and that even with several precautions; so that the fatal accident to Señor Silva Jardim, who lost his life here but a few hours after our departure, is not to be wondered at.

On looking down some 45 to 50 m. beneath us, we could see the glow from a mouth some 2 or 3 m. in diameter. The walls of the crater were concave, so that although overhanging at the top, yet a plumb-line let fall from the edge would strike the bottom of the cliff. The crater bottom was roughly plain, due to the combination of a talus all round, and an attempt at a cone encircling the main vent. It will be thus seen that the crater cavity was of the form of a convex-sided cylinder, or more simply barrel-shaped, with its upper diameter some 50 to 55 m.

With much difficulty we made our way around to the north side of the cone of eruption, which had now lost its usual loose scoria surface, which was buried beneath a thick coat of sand and dust, covered with a thin saline crust on its surface. The upper limit of the radial rift, which we were prevented from examining three weeks previously, on account of its giving out so much vapour as to constitute the temporary escape aperture of the volcano, had now become quiescent, so that we could fully examine it. Only a current of hot air was now issuing from it, but I was able to collect some fine masses of crystallized molysite and kremersite from its edges. Its average breadth was about 0·50 m., where it traversed old compact lava, but of course it disappeared as soon as it reached the looser materials. The real azimuth of its orientation, which we could now determine with greater accuracy than when we were walking over hot rock and enveloped in hot irritating vapours, proves to be, as it radiates away from the axis of Vesuvius, about 15° west of north. It curves then a little to the north, and near the foot of the great cone it again assumes nearly the same azimuth as at starting, an arrangement which is quite evident when the Vesuvian cone is regarded from the Punta del Nasone. From that, the highest point of Somma, the lower extremity of the rift lies a little to the right or west, and faces that part of the Somma ridge which corresponds to the upper extremity of the Vallone Cancherone.

In the forenoon of June 30 much dust had fallen at the lower railway station, of which we collected some bagsful. It is the usual fine sandy material of these eruptions, and consists of the pulverized materials of the cone of eruption.

Having passed the night at the lower railway station, the next day we crossed the Atrio, ascended to the western extremity of the ridge of Somma, and followed it along so as to get a general bird's-eye view of the whole scene of the eruption, and take photographs of the more important points. As one stands on the Punta del Nasone and embraces that magnificent view of Vesuvius and the Atrio del Cavallo, one sees at their feet the new lava-stream in the form of the letter L, the horizontal portion of which is still being prolonged down the Atrio towards the Fossa della Vetrana. In the middle of the ridge we found a thin coating of fine red dust which had reached thus far from the crater. Much of the Atrio was also covered by the same material. Scaling the cliff face just beyond the Cognulo di Ottajano to the Atrio del Cavallo, we again visited the lower point of the outburst. Most of the beautiful fumaroles were in a state of ruin, and lined by good-sized crystals of hæmatite and mixed chloride crusts. Here the lava was quite solid, though at one point was a hole, some 50 m. from the base of the great cone, where we could see the molten rock flowing lazily along about a metre beneath our feet. The lava at the end of the flow was making considerable progress to the westwards, and stood opposite dyke 13.

Since then, few changes have taken place in the mountain: the crater still gets larger, dust is thrown out, and the lava descends. These phenomena are capable of continuing for months if the drainage opening does not enlarge.

As the eruption progresses, I will send you further details. H. J. JOHNSTON-LAVIS.

THE PRODUCTION OF MUSICAL NOTES FROM NON-MUSICAL SANDS.

THAT I have succeeded in producing musical notes from sand that was never before musical, and am also able to produce similar results from certain mute or "killed" musical sands which have been temporarily deprived of their musical properties, has already been announced in the *Chemical News* (vol. lxiv. No. 1650).

It is not necessary now to give the details of the numerous experiments which led up to this discovery; it will be, perhaps, sufficient for present purposes, to state that in November 1888 I published a paper[1] in which I propounded a theory to account for the cause of musical sounds issuing from certain sands. After giving various reasons for my conclusions, I said:—"It occurred to me, then, that the music from sand was simply the result of the *rubbing together* of the surfaces of millions of perfectly clean grains of quartz, free from angularities, roughness, or adherent matter, in the form of clinging fragments investing the grains, and that these microlithic emissions of sound, though individually inaudible, might in combination produce a note sufficiently powerful to be sensible to us."

Having described numerous experiments, and drawn attention to the hopeful results obtained from the "millet-seed" sand, my paper concluded with the following:—"From what I have now told you, I think we may conclude that music may be produced from sand if (1) the grains are rounded, polished, and free from fine fragments; (2) if they have a sufficient amount of 'play' to enable them to slide one against the other; (3) if the grains are perfectly clean; and (4) if they possess a certain degree of uniformity in size, and are within a certain range of size."

On June 20 last I visited Studland Bay for the purpose of carrying out some new experiments. I found that the musical patch emitted tones louder and more pronounced than I had ever heard them there before. The best results were obtained by drawing a thick deal rod, on to the end of which I had fixed a resonator, over the surface of the sand; sounds produced in this way were heard unmistakably for a considerable distance. The patch averaged 7½ yards in width, and ran parallel with the trend of the shore for some hundreds of yards. The sand on the sea side of the patch was fine, and emitted notes of a high pitch; that on the land side was coarse, and emitted notes of a lower pitch. The rod drawn across the patch gave, therefore, a great variety of pitch. Many other interesting facts cannot now be referred to, but it is important to state that some of this sand, when taken off the patch, and struck in a box, gave out notes as it did *in situ*. On trying this sand subsequently at home, the *coarse* emitted distinct

[1] Read before the Bournemouth Society of Natural Science.

notes of a low pitch, but the fine was mute. This was, so far as I know, the first time that the Studland sand had been musical *off the patch*.

According to my theory, if the number of grains with polished surfaces could be increased in this fine sand, the number of vibrations would increase also, and so intensify the note, and cause it to become audible; this could only be done, however, by introducing a certain percentage of grains fulfilling the required conditions. To obtain such grains and to introduce them gradually until the necessary number should have been added, would have been a tedious process; and it occurred to me then that the same result might be obtained if the sand were struck in a vessel with a hard and polished interior. I placed, therefore, this fine sand in a teacup, and on striking it, found that it emitted a high, shrill note (A in *altissimo*), which was far more intense than that given when it formed a part of the patch.

When polished grains of sand are in contact with the sides and bottom of a glazed porcelain vessel, it is obvious that there are numerous points of contact between two polished surfaces—the sand grains and the vessel—and that on striking the surface of the sand, the friction necessary to produce the vibrations of a musical note is induced between these points.

This I *proved* by placing the same sand in various vessels with rough interiors, and by lining these glazed or polished vessels with silk, &c., but in no case would this sand emit notes unless the grains were in direct contact with the glazed or polished surfaces. This peculiarity is not in any way dependent upon the sonorous properties of the vessel used, for it may be "deadened" with impunity, and the note will remain unaltered.

The results of numerous experiments show that musical sand of the Eigg type—*i.e.* sand possessing in great perfection the physical conditions necessary for the production of music—will be musical in receptacles of whatever composition or form, though in some of these it emits notes "under protest" only.[1]

Those sands which are of the Studland Bay type—*i.e.* having the necessary physical conditions less perfectly developed, and are usually mute except *in situ*—will emit music only in vessels possessing hard and glazed interiors, and, as a rule, of a certain form; while some of the more "sulky" types of sand not only need a vessel of hard and glazed interior, and definite form, but also require a box, or small pedestal of wood (which I call a "coaxer"), on which this vessel must stand before the notes emitted become audible. A "sulky" sand was rendered far more musical by being sifted, washed, and boiled, giving out, after this treatment, notes without the aid of the "coaxer."[2]

After discovering what could be done with such simple apparatus, it occurred to me to try, under similar conditions, some of my abandoned sands—those unmusical sands that had been, during a period of four or five years, treated unsuccessfully for music.

One sand (an iron-sand composed of more or less polished grains, quartz, and much dust formed of denser minerals) gave a very hopeful "swish" (explained in my paper of 1888) in a certain porcelain vessel, and from this—by (1) sifting in sieves, to eliminate the fine material, and to insure uniformity in size of grain; (2) rolling down an inclined plane of frosted glass, to separate the rounded grains from the angular quartz; and (3) boiling in dilute hydrochloric acid, to cleanse the surfaces—I succeeded in producing a sand that, in certain glazed vessels, emits musical notes as clear as those emitted from any of my musical sands but that of Eigg. This sand gives F in *altissimo*, but it very soon becomes "killed" because of the fine dust and loss of polish that is the inevitable result of the attrition of the grains. There remains but one thing to be done, and that is to produce a sand which, like that of Eigg, will be musical in almost any receptacle, and I have reason now to think that this will not be very difficult.

It has not been possible here to record more than the merest outline of what has been done, or to give instances of the interesting *capriciousness* of these sands; it should be understood, however, that no *ordinary* beach or cliff sand has the slightest inclination to "sing" under any of the "coaxing" methods at present known to me.

CECIL CARUS-WILSON.

[1] When musical sands sound "under protest" they give out high, shrill notes. The smallest quantity of musical sand from which I can obtain a true note is a thimbleful of the Eigg sand. Small quantities emit notes of a high pitch.

[2] Many musical sands are quickly "killed" by constant striking, because the harder minerals present abrade the softer as they rub together, and this forms a fine dust.

NOTES.

SIR MICHAEL HICKS-BEACH, who previously gave a negative answer to the request made by the Executive Committee of the British Institute of Preventive Medicine, having reconsidered his decision, has now granted the required license to register the Institution as a Limited Liability Company, with the omission of the word "Limited." The license, however, is not to be construed as expressing approval by the President of the Board of Trade of experiments on animals, or in any way affecting the exercise by the Secretary of State of his discretionary powers to grant a vivisection license to the proposed Institute. The articles of association have been signed, and the Institute is now duly registered. The following gentlemen have already expressed their willingness to serve on the Council: Sir Joseph Lister, Chairman, Sir Charles A. Cameron, Mr. Watson Cheyne, Prof. Michael Foster, Prof. Greenfield, Prof. Victor Horsley, Sir William Roberts, Sir Henry Roscoe, Prof. Roy, Prof. Burdon-Sanderson, Dr. Pye-Smith. Dr. Armand Ruffer, of 19 Iddesleigh Mansions, Westminster, S.W., will act as honorary secretary until the first meeting of the Council.

THE graduation ceremony at the close of the summer session of the University of Edinburgh was held on Monday. Principal Sir William Muir, Vice-Chancellor, presided. Prof. Kirkpatrick presented for the honorary degree of Doctor of Laws Colonel Sir Colin Campbell Scott Moncrieff, K.C.M.G., C.S.I., R.E., remarking that, through his work as chief officer of the irrigation works of the Nile, it could be said that Sir Colin had created a greater and an infinitely freer, happier, and more prosperous Egypt than it was before. As a gallant officer, a distinguished man of science, a statesman of high merit, and, above all, as a benefactor of his fellow-creatures, Sir Colin was pre-eminently worthy of the highest of their academic honours. The honorary degree of Doctor of Law was then conferred *in absentiâ* on Prof. Simon Newcomb, Washington.

SIR JOSEPH FAYRER has been elected a Corresponding Member of the Royal Italian Society of Hygiene. Sir Joseph has also been promoted from the grade of Foreign Corresponding Member to that of Foreign Associate of the French Academy of Medicine.

PROF. DU BOIS-REYMOND, the distinguished physiologist of Berlin, has been awarded the Gold Medal for Science.

MR. J. E. KEELER has been elected Professor of Astrophysics in the Western University of Pennsylvania, and Director of the Allegheny Observatory. Mr. F. W. Very is associated with him as Adjunct Professor of Astronomy. It is expected that the Observatory will continue its researches on important problems in the domain of astro-physics.

IT is stated that Siam, following the example of Japan, is commencing to Europeanize her institutions. The founding of

a University has been decided upon, and Prof. Haase, of Königsberg (Germany), has accepted the appointment to the Chair of Physics.

THE last number of the *Rendiconti* of the Reale Accademia dei Lincei contains an account of the annual meeting held on June 9, at which the King of Italy was present. After the opening speech of the President, Brioschi, one of the chief features was an admirable address by Prof. Messedaglia on the Homeric uranology, with special reference to precession.

La Revue Scientifique of the 1st instant contains the address by M. Villemin, the President of the Tuberculosis Congress. It deals with recent researches. The results of the first Congress are also detailed by M. Petit, the General Secretary.

A FINAL meeting of the Committee of the Virchow Testimonial Fund took place on July 16, Sir James Paget, Bart., F.R.S., in the chair. The Treasurer gave an account of the moneys received, which amounted to about £175. It was resolved to send this sum to the General Treasurer of the Fund, and to present Prof. Virchow on the occasion of his birthday with an illuminated address, conveying to him the congratulations of the Committee and subscribers. This the Honorary Secretaries, Dr. Semon and Mr. Horsley, were directed personally to transmit to Berlin on the occasion of the celebration.

THE Essex County Council has appointed an Organizing Joint Committee, consisting of six members of their own body and six members of the Essex Field Club, to form a centre for supplying lecturers and teachers (with apparatus and materials), conducting examinations, and affording help and guidance to local bodies, in connection with the recent grants towards technical instruction. A grant of £900 has been made for these purposes. The members of the Committee are: (representing the County Council) Mr. E. N. Buxton, Mr. E. A. Fitch, Mr. J. H. Burrows, Mr. S. W. Squier, Mr. F. West, and Mr. W. B. Whittingham; (for the Essex Field Club) Prof. Boulger, Mr. F. Chancellor, Prof. R. Meldola, F.R.S., Sir Henry E. Roscoe, M.P., F.R.S., Mr. F. W. Rudler, and Mr. J. C. Shenstone. The Organizing Secretary to the Committee is Mr. W. Cole, 35 New Broad Street, E.C.

THE idea of "a British Museum of Portraits," to be executed by photography, was conceived as long ago as 1864 by Mr. James Glaisher, F.R.S., and brought before a meeting of the Council of the Amateur Photographic Association, of which the Prince of Wales is the President. The suggestion was cordially approved by the meeting, and photographs were taken in *carte de visite* size and deposited at the South Kensington Museum. At first, however, only fading silver prints were made, and these were so unsatisfactory that for some years the undertaking was held in abeyance. By the discovery and perfection of the process of permanent carbon printing, an opportunity has at length been afforded of resuming the prosecution of the work under infinitely more favourable conditions; and, as a result, a collection of excellent portraits is now being made by the Amateur Photographic Association. Already there are nearly 200 large permanent carbon portraits deposited in the Art Department at the South Kensington Museum, and about as many more are ready to be sent. These latter were on exhibition at a private view on Saturday last at 58 Pall Mall, S.W., the studio of Mr. Arthur J. Melhuish (Photographer Royal). They embrace some photographs of men of distinction in science, and are excellent both as likenesses and as specimens of photographic art. The conditions under which they are taken are, in fact, sufficiently exacting to insure the production of a faithful portrait, inasmuch as every portrait must be approved by the sitter and by the Standing Committee previous to its being placed in the South Kensington Museum. The undertaking is on a non-commercial basis, the photographs being taken for the purposes of this collection only, and not for publication, and no expense of any kind being incurred by the sitter. The invitations to sitters are issued under the authority of the Council.

THE Trustees of the Indian Museum, Calcutta, have just issued the second and concluding portion of a Catalogue of the specimens of Mammals contained in that Institution. The first volume of the Catalogue, compiled by Dr. John Anderson, the late Superintendent, was published in 1881. The present volume, which commences with the Rodents, has been prepared by Mr. W. L. Sclater, the present Deputy-Superintendent. The total number of specimens of Mammals contained in the Indian Museum, as is shown in the Catalogue, is 4872. These are referred to 590 species, of which, 276 are found within the limits of the Indian Empire, and the remainder are from elsewhere. As the Indian Museum contains many types of Blyth, Jerdon, and the older Indian authorities, the collection is one of considerable importance, and the Catalogue will be of much use to students of the group of Mammals.

FOR the first time for many years the *Journal für Ornithologie* has actually appeared within the month imprinted on the cover bearing the date of publication. English ornithologists have this year received in July the Heft bearing the date 'Juli, 1891.' *Gott sei dank.* The articles published in the present year appear also to be of a higher class than many of those formerly issued in the *Journal*, and some very important papers by Dr. Reichenow, Dr. A. B. Meyer, Herren Schalow, Hartert, &c., have been published. The chief interest centres round the collections which that greatest of modern naturalist-explorers, Emin Pacha, has sent to Berlin; and the birds obtained by him during his journey from Bagamoyo to Lake Tanganyika are fully described by Dr. Reichenow. The novelties are not many, but are sufficient to show that there is much to be done in German East Africa before our knowledge of its ornithology approaches completion. English naturalists will await with eagerness the zoological work of our Consul in Mozambique, Mr. H. H. Johnston, C.B., for the whole of the district in his sphere of influence is practically unexplored as far as natural history is concerned, and at present our knowledge is almost a blank. To Mr. Johnston and his companions, therefore, English zoologists are now looking for information which shall connect the work of Böhm and Emin with that of Kirk and Livingstone.

IN a recent paper to the Société des Ingénieurs Civils, M. Haubtmann states that in London the cost of the electric "horse hour" is 0·375 francs, that is three times the cost of gas. In Paris it is 0·90 francs, and at Saint Brieuc, the town where, since June 1 last, it is cheapest in France, it is still 0·52 francs. At Fribourg it has the lowest cost in Europe, 0·15 francs, and 0·10 francs for a consumption over 20 horse-power. Such differences, he points out, do not arise from difference in cost of motor force, for, deducting that, the horse-hour still remains in Paris at 0·75 francs, while in Fribourg it is 0·125 francs. They arise from differences in the amounts of capital engaged, and in the systems adopted.

IT is stated that a memorial is about to be presented to the United States Congress asking for the creation of a Government Department of Public Health, with a Cabinet officer at its head, to be known as the Medical Secretary of Public Health.

THE Danish Academy of Sciences has recently offered the following among other prizes:—A gold medal, worth about £17, for an exposition of the theory of electric vibrations in limited and resting bodies in general, with a special application to simple forms of perfect conductors, so that for these cases, the mathematical problem may be explained, and if possible solved. A prize of about £22, for an investigation showing in the case

of our four principal cereals, the nature, and as far as possible the proportional quantities, of the chief carbohydrates found at different stages of ripeness. Memoirs to be accompanied with preparations. A prize of about £27 for a complete account, accompanied with preparations, of the *Phytoptœcidia* found in Denmark, and a monographic exposition of the species of the genus *Phytoptus* (in its old and wider sense), which inhabit the various galls, found on a particular plant, with the view especially of showing whether several usually different galls of the same plant species arise from the same *Phytoptus* in different phases of its development. In choosing a plant, preference should be given for one in which these galls have an economic value, as is the case, *e.g.*, with some occurring on the beech. Further, the Academy desires an exposition, as complete as possible, of the development of a particular species of *Phytoptus*. The date for the first is October 31, 1892; for the two others October 31, 1893. Memoirs may be written in Danish, Swedish, English, German, French, or Latin.

THE fürstlich Jablonowsky Gesellschaft, recognizing the fact that the determinations of the secular perturbations of the orbits of the interior planets, in the form in which they have been left by Le Verrier, are not satisfactory, and that probably the anomaly in the motion of the perihelion of Mercury is to be explained by the fact that the differential equations have been treated linearally, offers a prize of 1000 marks for a new determination of the secular perturbations of the orbits of Mercury, Venus, the Earth, and Mars, in which the terms of a higher order are taken into account. Competitors are to send in the results of their investigations before November 1894, observing the usual rules to secure the anonymity of their papers.

THE *Educational Times* states that the Supreme Council of Hygiene of Austria has been engaged in discussing the advantages of erect as compared with slanting writing, and the official Report of Drs. von Reuss and Lorenz points strongly in favour of the former. They point out that the direction of the written characters has a marked influence on the position of the body. In "straight" writing the scholar faces his work, and is spared the twist of the body and neck, which is always observable in those who write slantwise, and one common cause of spinal curvature is thus obviated. The erect method is, therefore, expressly recommended for use in schools, in preference to the ordinary sloping lines.

WE have received the eighteenth Annual Report of the Geological and Natural History Survey of Minnesota. It consists of a summary statement for 1889, report of field observations made in 1888 and 1889, by N. H. Winchell; American opinion on the older rocks, by A. Winchell; additions to the library of the Survey since 1884, and a list of publications of the Survey.

L'Électricité points out that the new electric photophone, which consists of a small glow lamp at the end of an elastic tube used for throwing a strong light for surgical purposes into the mouth, ear, &c., was really suggested by the action of the water jet in the luminous fountains now so common, and that these really owe their origin to a laboratory experiment by M. Becquerel in 1876.

HERR KLENZE, we learn from a German source, has been making inquiry into the digestibility of different kinds of cheese. The most easily digested, he found, were Cheshire and Roquefort; while others are ranked as follows, in ascending order of difficult digestion: Emmenthal, Gorgonzola, Neuchâtel, Ramadour, Rotenburg, Mainz, fromage de Brie, and (most indigestible of all) Swiss cheese.

IN recent numbers of the *American Journal of Science* (February 1891) and *Ciel et Terre* (July 1 and 16, 1891) attention is drawn to the remarkable conclusions arrived at by Dr. Brückner in his work entitled "Klimaschwankungen"—the most complete work extant upon the question of the variation of climate—in which he shows that the climate has not undergone any continuous variation from the earliest historic time, but that it oscillates, and presents alternately periods of heat and cold, and of dryness and humidity, the period being about 35 years, which, it will be observed, is a multiple of the period of frequency of sun-spots (11 to 12 years). M. Penck, the eminent German geographer, has drawn some interesting conclusions as to the probable effects upon the harvests of the world.

PART 34 of Cassell's "New Popular Educator" has just been issued, and contains articles on applied mechanics, algebra, botany, electricity, and comparative anatomy.

MR. G. C. HOFFMANN, of the Geological and Natural History Survey of Canada, has made a microscopical and chemical examination of a peculiar form of metallic iron found on St. Joseph Island, Lake Huron. It appeared in the form of spherules disseminated through a thin deposit of dark reddish-brown limonite which coated certain faces of some surface specimens of quartz. These metallic-looking spherules were found to consist of nuclei of silicon coated with a humus-like substance, which in turn was overlain by a metallic layer containing all the elements most frequently met with in meteoric iron. But the small proportion of nickel present (0·11 per cent.), and the relatively large amount of phosphorus (1·07 per cent.), as also the fact that the spherules contain nuclei apparently of a concrete character, leads Mr. Hoffmann to suggest the possibility of a terrestrial source for the material, upon the assumption that it has resulted from the reduction of an iron-salt by organic matter. The paper, which is accompanied by four coloured plates, appears in the Transactions of the Royal Society of Canada, 1890.

THE preliminary results of some investigations upon the growth of the face are stated by Prof. G. M. West in *Science* for July 3. The values obtained in the case of measurements of the female face point to the existence of three distinct periods of growth, the first ending at about the seventh year, and the third beginning at about the age of fifteen. The abrupt transition from one period to the next is indicated by the very slow growth of some children until the ages of eight or fourteen, when a rapid development often occurs. From the fifth to the tenth year the average growth appears to be about 6·5 mm. During the next four years it is 6·2 mm., and from this time little advance is made, the maximum of 128 mm. being reached at about the age of twenty. The male face is larger than the female face at all ages. Its growth is also more rapid, and continues later in life. The measurements have been on 2500 persons, including both sexes.

PROF. TITO MARTINI, of Venice, contributes to the issue of the *Rivista Scientifico-Industriale* for the end of June, the results of some experiments on the crystallization of thin liquid films. He finds that a strong solution of sodium sulphate, when cooled to near its saturation point, possesses a viscous character which enables it to form a thin film on a metallic ring, as in Mr. Boys's experiments with soap-bubbles. On rapid evaporation such a film crystallizes to an extremely beautiful open lattice-work of minute crystals, which preserve their transparency for some time, and then effloresce and crumble to powder. The experiments succeeded with rings up to thirty-six millimetres diameter. Similar experiments with ammonium chloride and sodium hyposulphite have hitherto proved unsuccessful. With a transparent film of liquid sulphur, however, even more beautiful results have been obtained. The author regards such experiments, besides being eminently suitable for lecture demonstration, as likely to throw light on the nature of molecular arrangement in relation to crystallization.

THE same number of the *Rivista* summarizes a somewhat important communication to the Naples Royal Academy of Physical and Mathematical Sciences, in which Prof. Dino Padelletti urges that the usual investigation for the movement of the plane of oscillation of Foucault's pendulum in relation to the earth's rotation is insufficient. The author contends that the problem for latitudes between the pole and equator is more difficult than would appear from the usual simple solution, and cannot be solved by the principle of inertia. He proposes an equation derived from the principle of composition of the rotational forces.

A METEOROLOGICAL journal in the Russian language, the *Meteorologitscheskij Westnik* (Messenger), has lately appeared under the competent editorship of Woeikof, Rykatschew, and Spindler; its general plan seems to be like that of the German *Zeitschrift*. The idea of starting it arose at a meeting of the Russian Naturalists and Physicians at St. Petersburg in the end of 1889. Four graphic tables are given in this journal, showing the course of the meteorological elements during 1889 at the agricultural experimental station of Sapolje, also measurements of ground temperature, &c.

THE *Selborne Society's Magazine* for July contains the first of a series of articles on the Kew Museums by Mr. J. R. Jackson; others on the effects of environment on plants, and other interesting matter. Among the correspondence are complaints from Warwickshire that the Wild Birds Preservation Act is a dead letter there, as the "authorities," whoever they may be, take no trouble in the matter. On the other hand, the inhabitants of Shetland are fully alive to it.

THE last volume (xxii., 6) of the *Trudy* of the Society of Naturalists of Kazan contains the second part of Mr. Korzchinsky's valuable researches into the northern limits of the black-earth steppe region of East Russia. In the first part published in 1888, the author gave the results of his explorations in the province of Kazan. He now confirms his conclusions by further exploration in Samara, Simbirsk, Perm, and Ufa. He gives the northern limits of the black-earth steppe vegetation, and shows that they depend neither upon climate nor upon the altitudes, but chiefly upon the courses of the rivers.

ACCORDING to *La Nature*, the telephonic service of Paris, rapidly developing of late, will soon include an immense central telephonic office in the Rue Gutenberg, capable of serving directly 30,000 subscribers, without connection with the other offices of the quarter. The work is being actively pushed forward. Cables are being laid in the sewers, an enlargement of which, at certain points, is rendered necessary. There were 7800 subscribers in Paris last October. Paris has now telephonic communication with Brussels, Marseilles, Lyons (which also communicates with Marseilles), Lille, Havre, Rouen, and London. Twenty-eight towns in France have a telephonic system. There are two in Algeria, in Algiers and Oran. Lille and Roubaix, Lille and Dunkirk are connected by telephone; and, ere long, connection will be formed between Lille, Valenciennes, Calais, and Fourmies, between Lyons and Saint Etienne, between Dieppe and Rouen, between Marseilles and Nice.

THE climate of the Greek island Cephalonia has been lately described by Dr. Partsch (*Petermann's Mitt.*). We note the following features. At Argostoli temperature reaches a maximum in July (25°·3 C.), whereas in Corfu and Patras it does so in August. With several days' calm and bright sunshine, in the bay, the air, laden with moisture, becomes unbearably hot and close. Yet the natives go but little to the wooded hills behind, where the temperature goes down sometimes to 15°·5 C. or lower. Mules bring down snow nightly, in summer, from covered pits in the hills, for supply of restaurants, &c. As to rain, there is a sharp contrast between the wet winter-half and the dry summer-half of the year. The annual rainfall (3½ years) was about 35 inches. The autumn rains are ushered in by severe thunderstorms. November and December are the wettest months, but about Christmas there is usually a short time of fine weather. March is extremely variable, and often very cold. With May begin the rainless months, and the drought is sometimes considerably over 100 days. Five months have sometimes passed with but a few slight showers. On this greatly depends the currant cultivation: a brief downpour may spoil the crop. Snow falls seldom in Argostoli, but often on the hills. Dew is plentiful in summer, but its salt precipitate is feared. Wind is greatest in winter, southerly winds prevailing, especially south-east. A hot south wind (the *lambaditta*) blows, rarely, in early summer, and with evil effects to vegetation. The fresh north-west wind (*maestro*) brings cumulus clouds on the hills.

MR. F. HOWARD COLLINS, the author of a useful epitome of Mr. Herbert Spencer's system of philosophy, has written a pamphlet in which he discusses the causes of the diminution of the jaw in the civilized races. In opposition to the views of Weismann, he contends that the phenomenon is due to "disuse"; and the argument, as he presents it, deserves to be seriously considered. Some time ago Mr. Collins sent to NATURE a letter in which he gave some account of the ideas which he now expounds more fully. In the preface to his pamphlet he seems to imply that the letter was not inserted because, according to a belief said to be current among certain biologists, the editor of NATURE is "more willing to publish letters contending that acquired faculties are not inherited than those contending that they are." Mr. Collins has too readily allowed himself to be influenced by the belief of "certain biologists." If he supposes that it is possible for the editor of NATURE to print all the letters sent to the paper for publication, he must have a very inadequate conception of an editor's functions.

To throw light on some physiological processes, Herr Hofmeister recently experimented (*Archiv für experim. Pathol.*) on the swelling of plates of gelatine in various solutions; the plates being taken out from time to time, dried, and weighed. With salt solutions of various concentration, the gain of weight was large in the first days, then gradually fell off, as in former experiments with pure water. The effect varied with the nature of the salt; and even with solutions holding the same number of molecules in 1000 parts water, the swelling varied as much as five-fold. This difference, it is pointed out, is related to attraction of the salt for water; the greater the attraction, the more difficult the entrance of water into the plate. But that this is not the only factor is proved by the swelling in pure water being always much less than that in the solutions. Experimenting with ordinary salt, the gain of weight proved to consist both of water and salt, both dependent (but differently) on concentration. With increase of the latter, the gain of water rises to a maximum (about 13 per cent.), then declines; but the gain of salt goes on always increasing proportionally to the concentration. The remarkable property salts have of increasing the gain of water beyond what occurs in pure water is also shown by indifferent organic substances, as cane-sugar and alcohol. Experiments were further made on swelling of gelatine plates in methyl-violet solutions, and with the result that the concentration of the solution in the plates was always much greater (over 30 times) than that in the solution presented. The colouring-matter is taken up in relatively much greater quantity than the water. Further, gelatine takes up somewhat more colouring-matter relatively from a dilute than from a concentrated solution. The forces concerned in these phenomena, and

which are neither purely mechanical nor chemical, Herr Hofmeister brings into analogy with those occurring in absorption of gases by liquids, the reciprocal solution of liquids, adsorption of gases on solid bodies, &c.

The *Photographic News* quotes the following from the *Scientific American*, December 9, 1848:—"*New Electrical Light.*—The inventors of a new electrical light, exhibited at the Western Literary Institution, Leicester Square, London, on its recent reopening under the new auspices, expect, it is said, to apply it generally to shop and street illumination, and they state that, while the conveying will cost no more than gas, the expense of illumination will be one-twelfth the price of the latter light. The current of electricity, in passing through the two pieces of charcoal which form the poles of the circuit, and are excluded from all access of air, gives, in this case, it is said, an intense and beautiful white light, with the effect of daylight, to a much greater extent than the lime does, and having this advantage, that it is sustained and continuous. If Messrs. Staite and Petrie can thus produce a steady and sustained light they have accomplished what has hitherto been the sole preventive to the substitution of galvanism for gas. The *Mechanics' Magazine* states that this one light completely eclipsed ten gas lights and an oxyhydrogen. The gas companies had better look out. The dissatisfaction of the public with their mismanagement may have begotten a rival destined to eclipse many more than merely ten of their gas lights."

With the view of certifying to the efficiency of teachers of public elementary schools to give instruction in woodwork in accordance with the provisions of the Code (1890), the City and Guilds of London Institute is prepared to issue certificates to qualified teachers of public elementary schools on the following conditions:—The candidates will be required to give evidence of having regularly attended during each of two sessions, a course of at least twenty practical wood-working lessons in a school or class certified by, and under an instructor approved by, the Institute. The candidates will further be required to pass an examination at the end of each year's course, to be conducted by examiners appointed by the Institute, and to pay a fee of five shillings for each examination. For the first year, candidates who have attended an advanced course of instruction will be exceptionally admitted to the second year's examination without having passed the first, and will be eligible for the teacher's certificate. The examination fee for such candidates will be ten shillings. The written examination will include questions founded on such subjects as the following:—*Woods.*—Places from which some of the commoner woods are obtained. Their characteristic properties and uses. The general structure of cone-bearing and leafy timber trees. The meaning of seasoning timber. Effects of shrinkage and warping. Identification of specimens of wood. The questions will be limited to oak, ash, elm, beech, mahogany, sycamore, basswood, white deal (spruce), red pine (Scotch fir), yellow pine.

Das Wetter for July reports a curious case of globular lightning which occurred at Berga, near Schlieben, in Germany, between 3 and 4 o'clock on the morning of July 1. The lightning entered the chimney and split into two parts, one portion ran along the rafters of the roof, and the other entered a bed-room occupied by a man with his wife and three children. The man, who was up, on account of the violence of the storm, saw the ball jump on to the bedstead, which it broke, and from there it slowly travelled to the opposite side of the room, and disappeared, with a loud crash, through the wall. None of the occupants were injured, further than being deafened for a short time.

The additions to the Zoological Society's Gardens during the past week include a Banded Ichneumon (*Herpestes fasciatus*) from West Africa, presented by Dr. Arthur Williams; a Black Stork (*Ciconia nigra*), European, presented by Lord Lilford, F.Z.S.; two Nilotic Crocodiles (*Crocodilus vulgaris*) from Africa, presented by Dr. Lester; two Black Storks (*Ciconia nigra*), European, two King Parrakeets (*Aprosmictus scapulatus*) from New South Wales, purchased; a Laughing Kingfisher (*Dacelo gigantea*) from Australia, deposited.

OUR ASTRONOMICAL COLUMN.

Researches on the Mean Density of the Earth.—The *Monthly Notices of the Royal Astronomical Society* for June contain a brief account by Prof. A. Cornu of the experiments M. Baille and himself have been making for some years to determine the mean density of the earth. The apparatus employed is fundamentally the same as that used by Cavendish. It consists of a horizontal aluminium rod, suspended by a torsion thread 4 metres long, carrying at each end a ball of copper, bismuth, iron, or platinum, and at its centre a vertical mirror reflecting the divisions on a millimetre-scale 5 metres away. Two globes of mercury are used to produce the torsion couple. The displacements of the scale-divisions are observed with a telescope, and indicate the angular displacements of the rod. The chief improvements which have been made upon the apparatus used by Cavendish, Baily, and Reich, are as follows:—(1) The length of rod connecting the suspended balls has been reduced to 0·50 metre, *i.e.* to a quarter the length adopted by the above-named observers. (2) The attracting masses have been reduced to 10 kilogrammes. Cavendish used masses weighing more than 140 pounds. And the method of using fixed globes which can be quickly filled with mercury has been advantageously substituted for the movable lead weights. (3) The complete oscillation of the balance arm is registered on a chronograph by observing and recording the transits of the reflected scale divisions. (4) The use of an annealed glass fibre to eliminate errors due to displacements of the zero point. (5) The screening from variations of electric potential by putting all parts of the apparatus in metallic connection with the earth. (6) The copper case protecting the balance arm is a good conductor of heat, and of sufficient thickness to eliminate the disturbances due to variations in temperature. The authors hope soon to obtain an estimation of the probable error of their measures, and to arrive at a definite result for the constant they are determining.

Parallax of P Ursæ Majoris.—Vol. xxxviii. of the "Astronomical Observations of the University Observatory of Königsberg" contains the heliometer observations of P Ursæ Majoris (Arg.-Oeltzen 11677) made by Dr. Julius Franz, from which he deduces the parallax $0''\cdot1002 \pm 0''\cdot0065$, or approximately $0''\cdot10 \pm 0''\cdot01$.

THE PROGRESS OF MEDICINE.

The Bournemouth meeting of the British Medical Association has been a great success, and a great deal of useful work and discussion has been recorded. Among the addresses we may refer to the President's (Dr. J. R. Thomson), on the present position of medical officers of health; of Dr. Lauder Brunton, on twenty-five years of medical progress; of Dr. J. Chiene, on rest as a therapeutic agent in surgery; and others on lunacy legislation, the uses and prospects of pathology, &c.

We make the following extracts from Dr. Brunton's address, which presents us with a most admirable and masterly analysis of recent progress:—

. . . Perhaps there is no period in the whole history of medicine in which such rapid changes have taken place as in the last five-and-twenty years. It is impossible to give anything like a complete account of these in the brief space of one hour, and I shall therefore restrict myself to a few of the more prominent points, and especially those that have come directly under my personal cognizance; for, like the man who made one-half of his fortune by attending to his own affairs and the other half by leaving other people's alone, I may probably utilize the time at my disposal best by speaking of what I know myself and leaving other things out.

Advances in Knowledge and Teaching due to Experimental Method.—These changes have occurred both in the profession

itself and also to some extent—in this country at least—in the education and training of the men who enter it. We notice, first, that a very great increase has occurred in the knowledge of the nature, causation, and treatment of diseases possessed by the profession as a whole, but perhaps a still greater gain is the general adoption of the experimental method by which most of our recent knowledge has been acquired, and from which we may hope for even greater advantages in the future. In correspondence with the acquirement of knowledge, we notice also a great alteration in the teaching of medicine, and especially prominent is the tendency to make such teaching practical instead of theoretical by training men to place their dependence upon objective facts, and not to receive without experimental data the theories or speculations of any master, however great he may be. . . .

Direction of Advance.—The greatest advance made in the last twenty-five years has been in the direction of the accumulation, co-ordination, and teaching of facts instead of theories, of the phenomena of Nature as opposed to the fancies of the human mind.

Co-ordination of Facts.—But the mere accumulation of facts is of little use unless they can be so arranged, compared, and grouped as to bring them into relationship with some general law, and this we find in the world's history has been done from time to time by some master-mind. . . .

Influence of Darwin.—Medicine, both in its principles and practice, is really a subdivision of biology, and this, like all other branches of knowledge, has been most profoundly modified by the general acceptance of Darwin's great thoughts—the doctrine of evolution, the struggle for existence, and the survival of the fittest. Wherever we turn we find that Darwin's influence has modified the direction of thought, and whether the study concerns the evolution of the elements, the evolution of the planetary systems, of living beings, of communities, of customs, of laws, of literature, science, or art, in every department of human knowledge we find that men, consciously or unconsciously, are influenced by Darwin's work. It is with shame I confess that five-and-twenty years ago, although I had taken a University degree not only in medicine but in science, and might therefore be supposed to be acquainted with his work, I did not even know of the existence of his "Origin of Species," and I first heard its name in Vienna from the lips of an Austrian who was speaking of it in terms of the highest praise. "What is it?" I asked, and my question then seemed to cause my foreign friend as much astonishment as it causes myself now, when the possibility of such ignorance seems to me, as it must to you, almost incredible, and yet such was the fact. The publication of Darwin's "Origin of Species," in 1859, has done more to change the current of human thought than anything else for centuries, but while its influence is everywhere felt, biology and all its subdivisions have been more especially affected.

Changes in Medical Students.—But great as the changes have been during the last five-and-twenty years in the profession itself, they are perhaps quite as great in the men who enter it. . . .

Long ago the doctor's means of diagnosis consisted in inspecting the tongue, feeling the skin, counting the pulse, shaking the urine, and looking at the motions and the sputum. But now, in addition to a thorough training in auscultation and percussion, students have to learn the use of the laryngoscope, ophthalmoscope, and otoscope, and the application of electricity. They have to acquire a knowledge of the chemistry of the urine and its alterations in disease, and, what takes still more time, they have to learn the microscopical appearances, not only of the tissues and excretions in health, but their alterations in disease, and must be acquainted with the methods of staining so as to detect tubercle bacilli and other disease germs. . . .

Departments of Greatest Advance.—Five-and-twenty years ago we knew only too well that typhus was infectious, and that pyæmia and erysipelas were likely to spread in a ward when once they got into it, but we did not know then the causes of these diseases as we do now, nor had we the same means at our disposal wherewith to combat them. The departments in which the greatest advances have been made within the last five-and-twenty years are in those of fevers and diseases of the nervous system. A new era in the study of the latter was foreshadowed by the experiments of Fritsch and Hitzig on the brain of the dog, but it can only be said to have fairly begun with Ferrier's localization of the cortical centres, both motor and sensory, in the brain of monkeys. For the brain of the dog was too unlike that of man for experiments upon it to be of much practical use in the diagnosis of human ailments, while the likeness in the brain of the monkey to that of man at once allowed conclusions drawn from the experiments upon the former to be transferred upon the latter. Yet if we try to describe in one word the department in which medicine has made the greatest progress within the last quarter of a century, that word must be "fevers"; for during this time we have learned to recognize fever by the use of the thermometer in a way we never did before; we have learned the dependence of the febrile process in the great majority of cases upon the presence of microbes in the organism, and we have become acquainted with an immense number of chemical substances which have the power both to destroy the microbes and to regulate the febrile process.

Introduction of the Thermometer.—It is true that the thermometer was used by Danielssen, in leprosy, before the year 1848, and its more general use began with Wunderlich's observations nearly thirty years ago, but it is only within the last five-and-twenty years that its use has become at all general. . . .

Nature of Fever.—The thermometer has not only enabled us to detect the onset and to watch the progress of fever, but in conjunction with microscopical research, physiological experiment, and chemical analysis it has enabled us to gain a fuller knowledge of the nature of the febrile process itself. We know that during it the organism is consuming rapidly, or, as Dr. Donald MacAlister graphically says, it is like "a candle burning at both ends," and we have learned scientifically the reasons for the practical treatment, of which Graves was so proud that he wrote as his own epitaph, "He fed fevers." We have learned also, to a great extent, the necessity for the elimination of the waste products, or ashes as we may term them, which the excessive combustion produces, and thus we know why the surgeon is so anxious regarding the result of an operation when the kidneys of his patient are inadequate. For if any febrile attack following the operation should lead to increased demands upon these secreting powers, they might fail to meet it, and the retained excreta would poison the patient.

New Methods.—The rapid increase in our knowledge has been due not merely to the constant use of old methods, but to the introduction of new ones, and more especially to the general recognition of the fact that the same strategy which has often proved so successful in war is to be applied in attacking complex problems. They are to be separated as far as possible into their several components, and each of these is to be overcome in detail. As presented to us by observation at the bedside, the problems of disease are too complex for us to solve, and we are only succeeding in doing it by examining the various factors one by one in the laboratory. The greatly increased powers of the microscope and the better methods of illumination have been of the greatest service, but their utility would be very much less than it is had it not been for the general introduction of the microtome and the invention of new methods of staining. When I was a student the microtome was only used for cutting sections of wood in the class of practical botany. About that time it was employed by Mr. Stirling, Prof. Goodsir's assistant, in the preparation of animal tissues, but I believe that we owe its general introduction to Prof. Rutherford. The facility with which sections are made by it has made microscopical research much less tedious, and has enabled trained histologists to do more work in a given time, and medical students to acquire knowledge more rapidly. But without the method of staining introduced by Weigert and Ehrlich, we should, even with the best microscopes, be unable to recognize most of the microbes which are so important in the causation of disease.

Good Out of Evil.—It is very interesting to see how good may come out of evil, and a striking illustration of this is afforded by the history of medicine in the period we are now considering. For it seems to me that we can trace a great part of our knowledge of disease germs and of the antiseptic remedies we use in treatment to the cupidity and stupidity of the Spaniards of the Cordilleras. Their cupidity led them to cut down the cinchona trees of the Andes in order to fill their pockets with the gold they received in exchange for the precious bark, while their stupidity prevented them from planting new trees to replace those which they felled. The consequence of this was that quinine became so dear that it was evident that anyone who could produce it artificially

would make his fortune. Amongst others, Perkins tried to do this, and, although he failed, yet in the attempt he discovered the anilin dyes, whose staining powers have not only helped us so much in ordinary histological research, but have made it possible to distinguish disease germs which without them would have been invisible. But the discovery of the anilin colours was only one outcome of the attempt to make quinine synthetically, for the impulse which it gave to the study of aromatic compounds has led to the production of salicylic acid and acetanilide, antipyrin, phenacetin, and all the other antipyretic remedies whose number is probably legion, and whose names already have become so numerous as to be troublesome. Here we see good has arisen out of evil; for if the price of quinine had not been so high, the researches which have proved so useful might not have been begun even yet.

Small and Great, Foolish and Wise.—In looking at another of the greatest advances which medicine has made—namely, the knowledge of infective disease—we can see how enormous results can arise out of very small beginnings, and the safety of nations may be consequent upon a research which many men would have termed useless or even frivolous. I can hardly fancy any better illustration of St. Paul's observation about the foolish things of this world confounding the wise than Pasteur's researches on tartaric acid; for what could seem more foolish to the so-called practical man than the question, "Why does a crystal of tartaric acid sometimes take one shape and sometimes another?" Yet from an attempt to answer this question has arisen the whole of Pasteur's work on fermentation in general, and on that of wine, beer, and vinegar in particular, whereby he has been able to save millions to his country by accelerating the production of vinegar and preventing the souring of wine and beer. His observation that tartaric acid sometimes turned the ray of polarization to the right, sometimes to the left; that, indeed, there were two crystals apparently alike, but really different; and that these could be combined so as to form a symmetrical crystal having no power of rotation, led him to look to life and living beings as the source of asymmetry. He tried to produce this asymmetry in salts of tartaric acid by fermentation, and found that during the process an organism developed which eats up the dextro-tartaric acid, and leaves the lævo-tartaric acid behind. This led him to investigate such minute organisms, and, by simplifying the soil in which they grew, and separating the organisms one from another, he learned the conditions of their growth, and showed that most processes of fermentation were due to the presence of living organisms. It is true that while Pasteur was still a boy at school, Peyen and Persoz had shown that the liquefaction of starch and its conversion into sugar was due to diastase, and that Dumas in a report on a paper by Guérin-Varry had pointed out that, although unlike diastase, the active principle of the gastric juice had not been isolated, it was probably a ferment of a somewhat similar kind. Dumas classed yeast as a ferment along with diastase, and the fact that such a process as conversion of starch into sugar could be effected without a living organism naturally rendered it all the more difficult for Pasteur to prove his thesis that most fermentations were due to living organisms.

Chemical and Biological Views of Fermentation.—The two views of the action of ferments—namely, the chemical and the biological—may, I think, fitly be likened to Pasteur's two kinds of tartaric acid, each by itself being lopsided and incomplete, forming a symmetrical whole only when united. There can be no doubt of the truth of the chemical view that diastase is not a living organism, and yet converts starch into sugar. There can be as little doubt of the biological view that yeast and other organisms which cause fermentation are living bodies, and that without the presence of these living bodies alcoholic, acetic, and other forms of fermentation would not exist.

Microbes and Enzymes.—But recently we have come to recognize that these living organisms may produce their effect by manufacturing chemical ferments, and that these ferments may occasionally do the work, although the organisms which form them may be absent. It is quite true that it is difficult—perhaps impossible—to get fermentation from the dead yeast plant, but we may find a parallel for this in the fact that the pancreas of the higher animals sometimes yields an active ferment and sometimes not. Nor need we wonder that the ferments produced by microbes have but a slight action compared with those of the microbes themselves, if we remember how very little power of digestion a dead pig's stomach has as compared with the amount which can be digested not by the live animal itself only, but by the herds of swine consisting of its "fathers and mothers, its brothers and sisters, its cousins and its aunts," during all the term of their natural lives; for in the process of fermentation microbes are growing, fermenting, and dying with great rapidity, and many generations occur in a fermenting fluid in the space of a few hours, so that the total effect they produce will be out of all proportion to any which can be got from the microbes themselves at a single instant.

Microbes and Disease.—From organisms as a cause of fermentation and of the diseases of wine and beer, Pasteur went on to investigate their action as causes of disease in living beings—first in the silkworm, next in the lower animals, and, lastly, in man. He established the dependence of the silkworm disease and of anthrax upon the presence of specific microbes which could be transmitted and communicate the disease, and by destroying the infected eggs of the silkworm he eradicated the disease and restored the silk industry to France.

Weakening of Disease Germs.—But while this investigation is interesting to us as illustrating the probable cause of the disappearance of typhus fever, to which I have already alluded, Pasteur's researches on anthrax are still more important as bearing upon the question of protective inoculation; for he found that the disease germ could be cultivated outside the living body and grown in flasks under varying conditions, some of which were favourable and others unfavourable to its growth. High temperature enfeebled the virus, so that it no longer killed an animal with the same certainty, and by inoculating first with a weak virus and then with one successively stronger and stronger, he found that animals could be completely protected either from inoculation by the strongest virus or by infection from other animals suffering from the actual disease.

Increase in Virulence of Disease Germs.—Another extraordinary fact which he made out was that the virus thus weakened, so that it will not kill a guinea-pig a year old, and still less a sheep or ox, may again be rendered most potent by inoculating a feeble animal, such as a guinea-pig a day or two old, from this older and stronger guinea-pig's, the strength of the disease germs increasing with every inoculation, until finally sheep and cows may be killed by it. We can thus see how an epidemic of disease beginning sporadically, and attacking weak individuals, may gradually acquire such strength as to attack and carry off the strongest.

Pure Cultures.—Pasteur's plan of growing disease germs outside the body in broth, although of the utmost value, did not allow a convenient separation of different germs; but this can now readily be done by Koch's plan of sowing them, not in a liquid medium, but on solid gelatine spread on glass plates, so that the growth of the germs can be daily watched under the microscope, and inoculations made from single colonies on other plates until pure cultures have been obtained. By thus isolating the different microbes, we learn their life-history, the mode in which their growth is influenced by differences of soil, of temperature, of moisture, by the addition of various substances which either favour or retard their growth, and, last but not least, the effect which one microbe has upon another when they are grown together at the same time.

Struggle for Existence amongst Microbes.—For even amongst these minute organisms the struggle for existence and the survival of the fittest exists, like that which Darwin pointed out so clearly in the case of higher plants and animals. . . .

Struggle for Existence between Microbes and the Organism.—But it is not merely between different species of microbes or different cells in an organism that this struggle occurs. It takes place also between the disease germs and the cells of the organism which they invade, and the result of the struggle may be determined, not by some powerful agency which weakens or destroys either the organism or the microbe, but by some little thing which simply inclines the scale in favour of one or the other. Thus, in the potato disease, the victory of the invading microbe and the destruction of the potato, or the death of the microbe and the health of the tuber, may depend upon some condition of moisture or possibly of electrical change in the atmosphere which aids the growth of the microbe disproportionately to that of the potato. These atmospheric conditions need not necessarily be antagonistic to the potato, they may even in themselves be advantageous to it; but if they help the microbe more than the plant, the microbe will gain the victory and the plant be destroyed.

Fight between Cells in Higher Organisms.—The fight between

the organs which Æsop describes in his fables actually occurs between the cells in some vertebrate animals, and the schism predicted by St. Paul as the result of such a fight actually takes place. For in the tadpole, at one stage of its existence some of the cells at the base of the tail begin to eat up others, with the result that schism occurs and the tail falls off.

Phagocytosis.—This struggle for existence between the cells of an organism and microbes has been beautifully shown by Metschnikoff in the Daphne or water flea, where the process of the cells eating up the microbes or the microbes destroying the cells can be actually observed under the microscope. This process of phagocytosis is now regarded by many as only a small part of the struggle between an organism and a microbe, but it is impossible to see one part of a microbe half digested by the cell in which it is embedded, while the part outside remains unaltered, without believing that the process is one of great importance. At the same time, it seems that the process of phagocytosis, where the microbe and the cells meet in close conflict, bears about the same relationship to the total struggle that a bayonet charge bears to a modern battle. The main part of the fight is really carried on at some distance by deadly weapons—by bullets in the case of the soldier, and by ferments, poisonous albumoses, and alkaloids on the part of the cells and the microbes. In some of Metschnikoff's observations we can almost see this process, for he has figured leucocytes dead, and apparently burst by the action of conidia, lying close to but yet outside them, as if these conidia, like the dragons of fable, had spit out some venom which had destroyed them.

Venom of Microbes.—Within the last few years attention has been gradually becoming directed less to microscopical examination of the microbes themselves and more to chemical investigation of the ferments and poisons which they produce; yet, strangely enough, the very moment when chemistry is becoming more important than ever has been chosen to minimize the teaching of it in medical schools, and examination in it by licensing bodies. It is now possible to separate the albumoses and poisons from the microbes which produce them either by filtration, or by destroying the microbes by graduated heat; for, as a rule, they are destroyed by a lower temperature than the albumose or poisons which they form.

Microbes and Enzymes.—As the albumoses produced by microbes are nearly allied, chemically and physiologically, to those formed in the alimentary canal of the higher animals by digestive ferments, it is natural to suppose that microbes, like the higher animals, split up proteids, starches, and sugars by enzymes, which they secrete, and which in both cases may be obtained apart from the living organisms which produce them; that, in fact, we should be able to isolate from microbes bodies which correspond to pepsin or trypsin, just as we can isolate these from the stomach or pancreas of an animal. In some, although not in all cases, this attempt has succeeded.[1]

Poisonous Albumoses.—The albumoses produced by microbes resemble those formed during normal digestion in being poisonous when injected directly into the circulation, although they may not be so greatly absorbed from the intestinal canal. One of the most remarkable discoveries in regard to albuminous bodies is the fact that some of them which are perfectly innocuous, and, indeed, probably advantageous to the organism in their own place, become most deadly poisons when they get out of it. Thus, the thyroid and thymus glands, which are perfectly harmless and probably useful, were found by Wooldridge, when broken up in water, to yield a proteid which instantaneously coagulated the blood if injected into a vein, so that the animal died as if struck by lightning; while Schmidt-Mühlheim, under Ludwig's direction, found that peptones had an exactly opposite effect, and prevented coagulation altogether.

Neutralization of Poisonous Albumoses.—Perhaps the analogy is too vague, but we seem to find here something very like Pasteur's two kinds of tartaric acid, one rotating polarized light to the right, the other to the left; but, when united together, having no action at all, for here we have two bodies, one of which destroys coagulability entirely, the other increases it enormously; while many albuminous bodies have no action upon coagulation whatever. This view would lead us to suppose that one form of albumose may neutralize the action of another, thus rendering them both completely innocuous, whilst either one or other alone might be a deadly poison. The albumoses formed by microbes appear frequently, if not always, to have a double action, destructive and protective, on the higher animals. Pasteur's treatment of hydrophobia is based on the idea that the spinal cord of rabid animals contains a virus, and its antidote—Koch's tuberculin—may be similar in this respect, and may yet, by suitable alterations, fulfil the hopes of its able and single-minded discoverer.

Zymogens and Enzymes.—Perhaps a similar process of splitting up and recombination may explain the formation and disappearance of the enzymes, such as pepsin and trypsin, by which digestion is carried on. The pancreas of a fasting animal will not digest albuminous bodies like fibrin, while the pancreas of an animal killed during full digestion will do so rapidly. Yet the fasting pancreas contains the zymogen, or mother substance, which yields the digestive ferment, and, as Kühne has shown, by treating it first with acid and then with alkali, it becomes active. Again, to recur to the analogy of Pasteur's tartaric acid, we seem to find that the inactive, and possibly symmetrical, albuminous substance of the fasting pancreas is split up by this treatment after death or during the process of digestion in life, and yields the lopsided and active pancreatic ferment. But, if this be so, what becomes of the other half which has been split off? We do not at present know, but curiously enough Lépine has lately shown that while the pancreas is pouring into the digestive canal a ferment which will form sugar, it is at the same time pouring into the circulation another ferment which will destroy sugar.

Immunity.—We must be very careful in our speculations, and test them by experiment, but such observations as these may tend to throw some light upon the nature of immunity. Immunity is probably a very complex condition, and is not dependent altogether upon any single factor, but we can now understand that if a microbe has gained an entrance into an organism, and produces a proteid or an albumose poisonous to the organism which it enters, it may grow, thrive, and destroy that organism, while the injection of some other proteid which would neutralize the poison might save the animal while the microbe would perish.

Cure of Anthrax.—Thus Hankin has found that, while a mouse inoculated with anthrax will die within twenty-four hours, a rat resists the poison altogether; but if the mouse after being inoculated with the disease has a few drops of rat's serum injected into it, instead of dying, as it would otherwise certainly do, it survives just like the rat, and from the spleen of the rat Hankin has isolated a proteid which has a similar protective action to that of the serum.

Cure for Tubercle.—Working on similar lines, Bernheim and Lépine used the injection of goat's blood in phthisis so as to stop, if possible, the progress of tubercle, and Richet has used the serum of dog's blood, for the goat is quite immune, and the dog is to a great extent, though not entirely, immune from attacks of tuberculosis. The injection of goat's blood in somewhat large quantities has been given up, while dog's and goat's serum in small quantities of 15 to 20 minims at intervals of several days is still under trial.

Action of Blisters.—But if immunity can be insured by such slight changes in the organism as a few drops of serum from a rat will produce in the body of a mouse, it is natural to suppose that a similar change might possibly be effected by removing the albuminous substance from one part of the body, and introducing it, perhaps after it has undergone slight change, into another. As I have already mentioned, the albumoses of ordinary digestion are poisonous when they are injected into the circulation, and so are the proteid substances obtained from the thyroid and thymus glands. Why, then, may not the serum of one's own blood, withdrawn from the vessels by a blister and reabsorbed again, not be as good as the serum obtained from the blood of an animal? . . .

Bleeding.—It is quite possible, too, that the good effects of bleeding may be due to a similar cause. . .

Speculation and Experiment.—The human body is a most complex piece of mechanism. We learn its action bit by bit very slowly indeed, and we are only too apt to regard the little piece which attracts attention at the moment as all-important and to leave the other parts out of sight. But this is not true of our study of the body only, for the same tendency manifests itself in the pursuit of knowledge of all kinds, yet it is in medicine more especially that this tendency comes to

[1] *Vide* Brunton and Macfayden, Croonian Lectures on "Chemical Structure and Physiological Action," *British Medical Journal*, June 15, 1889, p. 1336.

be a matter of life or death, for upon the medical view prevailing at the moment medical practice is apt to depend, and erroneous views may lead to the death of many patients. So long as practice depends upon theories, unchecked by experiment, so long will medical practice prove fluctuating, uncertain, and dangerous. One of the greatest gains of the last five-and-twenty years is the general introduction of the experimental method, and the habit which has been growing up during it of accepting no statement unless based upon experimental data. Speculations such as those in which I have been indulging in regard to blisters and blood-letting are useful as indicating lines of experimental research, but until these have been thus tested it is foolish and may be dangerous either to accept and act upon them as true or to scout them entirely as false and absurd. Imperfect knowledge is almost sure to lead to one-sided practice, and thus, diverging further and further from the truth, ends at last in falsehood and folly.

Antisepsis.—Perhaps no better example of this can be found than antiseptic surgery, from the time of the good Samaritan down to Ambroise Paré and Sir Joseph Lister. The good Samaritan bound up the wounds of the poor traveller, pouring in oil and wine, which, only a few years ago, was recommended in an Italian journal as an excellent antiseptic. Ambroise Paré, when his ointments ran out, could not sleep for thinking of the miserable soldiers to whom they had not been applied, and was greatly astonished to find in the morning that these wretched neglected ones were better and happier than their comrades who had been treated *secundum artem.* I have no doubt that Paré's predecessors, in trying to improve upon the methods of the good Samaritan and upon the still useful friars' balsam, which is a powerful antiseptic but stings the wound or sore, had tried to make their applications more and more irritating, not knowing that it was the antiseptic power and not the irritant qualities which were desired. Paré abolished the ointments with the irrritation they caused, and thus did great service to surgery. But a greater one yet was rendered by Lister when he recognized that the danger of operations was due to the entrance of germs, and by preventing this has completely revolutionized surgical practice; nay, more, he has to a great extent revolutionized medicine, for the diseases of the internal organs, which were formerly entirely under the physician's care, are now becoming amenable to surgical treatment, and diseases of the stomach, intestine, liver, kidney, and lungs, and even of the brain and spinal cord, are now successfully treated by surgery when medicines are powerless to help. The most remarkable of all the recent triumphs of surgical operations upon the brain in which Mr. Horsley has gained such well deserved fame, would have been impossible without Ferrier's localization of cortical centres, and would have been equally impossible but for Lister's antiseptic method.

Disinfection.—But it is not only in surgery that recognition of diseased germs as a source of danger to the organism has led to their destruction outside the body, and insured safety from their attack. This occurs in all infective diseases, and this term now includes many which were not formerly regarded as such, for neither consumption nor pneumonia was formerly regarded in this light; but just about twenty-five years ago tubercle was shown to be inoculable, and since then the discovery of the bacillus of tubercle by Koch, and of pneumonia by Friedländer, has caused us to class both these diseases as not only infective, but as caused by definite organisms.

Prevention of Epidemic Diseases.—So long as people were ignorant of the causes of epidemic diseases, they were utterly unable to combat them, and they either in fury slew defenceless people for poisoning the wells, as in the Middle Ages, or appointed days of fasting and prayer, as in our own times. But once an epidemic is known to depend upon the presence of a certain organism, precautions can be taken for destroying the organism outside the body by means of disinfectants, or for lessening the susceptibility of the organism to its ravages inside the body by inoculation, or combating its effects by means of antipyretics. A knowledge of the life-history of microbes has enabled us to ascertain the power of different substances, either to destroy them completely or to arrest or retard their germination and growth, and in this way to prevent the occurrence of the diseases which these microbes might otherwise produce. . . .

Antivivisection.—Every now and again a loud outcry is raised against this method, partly from ignorance and partly from prejudice. Many—probably most—of the opponents of experiments on animals are good, honest, kind-hearted people, who mean well, but either forget that man has rights against animals as well as animals against man, or are misled by the false statements of the other class. These are persons who, blinded by prejudice, regard human life and human suffering as of small importance compared with those of animals, who deny that a man is better than many sparrows, and who, to the question that was put of old, "How much, then, is a man better than a sheep?" would return the reply, "He is no better at all." Such people bring unfounded charges of cruelty against those who are striving, to the best of their ability, to lessen the pains of disease both in man and also in animals, for they, like us, are liable to disease, and, like us, they suffer from it. I may perhaps be allowed to quote two sentences from a paper which I wrote twenty-four years ago, and therefore a considerable time before any antivivisection agitation had arisen, for they expressed then, and they express now, the objects of experimental pharmacology:—"Few things are more distressing to a physician than to stand beside a suffering patient who is anxiously looking to him for that relief from pain which he feels himself utterly unable to afford. His sympathy for the sufferer, and the regret he feels for the impotence of his art, engrave the picture indelibly on his mind, and serve as a constant and urgent stimulus in his search after the causes of the pain, and the means by which it may be alleviated" (*Lancet*, July 27, 1867).

Gains by Experiment on Animals.—It is said that our mouths are full of promises, but our hands are empty of results. The answer to this is, that anyone who doubts the utility of experimentation upon animals should compare the *Pharmacopœia* of 1867 with our present one. To it we owe, in great measure, our power to lower temperature, for to it is due not only the introduction of new antipyretics, such as salicylate of soda, antipyrin, antifebrin, and phenacetin, but the extension of the use of quinine from a particular kind of fever—malaria—to other febrile conditions. To it also we owe our greatly increased power to lessen pain by the substances just mentioned, which have not only an antipyretic but an analgesic action, and give relief in the torturing pains of neuralgia and locomotor ataxy when even morphine fails to ease, unless pushed to complete narcosis. The sleeplessness, too, which is such a frightful complication in some fevers, can now be combated by other remedies than opium and antimony; and we have the bromides, chloral, sulphonal, paraldehyde, urethane, chloralamide, and others, which, either by themselves or added to opium, enable us to quiet the brain instead of exciting it to further action, as opium alone so frequently does. Our whole ideas regarding cardiac tonics also have undergone a complete revolution within the last quarter of a century, for I was told, when a student, that digitalis was a cardiac sedative, and was apt to depress the heart, whereas now we know that it and its congeners—strophanthus and erythrophlœum and spartein—increase the heart's strength, raise the vascular tension, and are useful not only in sustaining the circulation, but in aiding elimination. This view of the action of cardiac tonics, which has revolutionized the treatment of heart disease, we owe chiefly to the experiments of Traube, although my own experiments, made in the laboratory of Sir Douglas Maclagan under the direction and by the help of my teacher and friend, Dr. Arthur Gamgee, may have helped towards its general acceptance in this country.

Future of Pharmacology.—But perhaps the most promising thing about pharmacology is that we are now just beginning to gain such a knowledge of the relationship between chemical structure and physiological action that we can, to a certain extent, predict the action of a drug from its chemical structure, and are able to produce new chemical compounds having a general action such as we desire; for example, anæsthetics, soporifics, antipyretics, analgesics, although we have not yet arrived at the point of giving to each one the precise action which would make it most suitable in any particular case. Even when we do not know the chemical structure of a drug, we may be able, from noticing one of its actions, to infer that it possesses others. We are, indeed, getting a knowledge of the action of drugs both of known and unknown chemical structure, and a power of making new remedies which will, I believe, enable us within the next five-and-twenty years to cure our patients in a way that at present we hardly think. . . .

THE INSTITUTION OF MECHANICAL ENGINEERS.

THE summer meeting of the Institution of Mechanical Engineers was held at Liverpool last week, commencing on Tuesday, the 28th ult., and concluding on Friday, the 31st ult. The President of the Institution, Mr. Joseph Tomlinson, presided throughout, and the meeting was highly successful, the long and varied programme being carried out with regularity and precision. The sittings for reading papers were held on the mornings of Tuesday and Wednesday; the afternoons of those days and also the Thursday and Friday being devoted to excursions. We will first deal with the papers and discussions.

The sittings were held in the concert-room of St. George's Hall, and the following list of papers was on the agenda:—A review of marine engineering during the past decade, by Alfred Blechynden, of Barrow-in-Furness; description of the warehouse and machinery for the storage and transit of grain at the Alexandra Dock, Liverpool, by William Shapton, of London; on the experimental engine and the alternative testing machine in the Walker Engineering Laboratories of University College, Liverpool, by Prof. H. S. Hele Shaw, of Liverpool; on the mechanical appliances employed in the construction of the Manchester Ship Canal, by E. Leader Williams, Engineer-in-Chief to the Canal Company. There was also a paper on the Liverpool water-works, but this was adjourned to the next meeting.

The Institution having been welcomed to Liverpool by the Mayor, Mr. J. B. Morgan, and the formal business having been transacted, Mr. Blechynden's paper was read. Mr. Blechynden has taken up the work commenced by Sir Frederick Bramwell at the Liverpool meeting of 1872, when the latter presented an historical paper giving a review of marine engineering up to that time. In 1881, the Institution met at Newcastle, when Mr. F. C. Marshall, a well-known Tyneside engineer, read a paper which consisted of a retrospect of the nine years since Sir Frederick Bramwell's paper had been read. We now have Mr. Blechynden carrying on the work. These periodical reviews are instructive. They cause the engineer to take stock of progress made, and enable him to see the lines upon which improvement may be expected to travel in the immediate future. Mr. Blechynden has been fortunate in the period which has fallen to his lot to review, for during the ten years past the triple compound engine has been developed. When Mr. Marshall read his paper, the ordinary compound engine with two cylinders was all but universal for steamships. Boiler pressures averaged 77·45 pounds per square inch, the average piston speed was 467 feet per minute, and the heating surface per indicated horse-power was 3·99 square feet. The consumption of coal per indicated horse-power was 1·828 pounds per hour. As a contrast to this, Mr. Blechynden tells us that at the present time the three-stage expansion engine has become the rule, and the boiler pressure has been increased to 160 pounds, and even as high as 200 pounds per square inch. Four-stage expansion engines of various forms have also been adopted. Forced draught has come to the front—largely, it would seem for the purpose of being abused—tbe piston speed has risen to 529 feet per minute, the heating surface per indicated horse-power is 3·274 square feet, and the coal consumption per indicated horse-power per hour is 1·522 pounds. By these figures it will be seen that during the last ten years the working pressure has about doubled, and that fuel economy has been improved by about 20 per cent. We may say that we do not always place full reliance in the details given with regard to fuel economy in connection with mercantile marine engines. We think that the power is apt to be taken on the best performance of the engines, so that they are credited with a duty they cannot maintain continuously throughout a voyage. Probably, however, the figures given by the author are accurate for comparative purposes, and they are not, as are some results claimed by marine engineers, altogether too good to be true. We would here draw attention to the author's expressions "three-stage" and "four-stage" compound engines. Engineers have been in the habit of referring to these types as triple expansion and quadruple expansion engines. This nomenclature is inaccurate and misleading for an ordinary two-cylinder compound, and even the simple non-compound engine expands the steam more than three or four times. Some engineers, recognizing this, have used the terms "triple compound" or "quadruple compound," but Mr. Blechynden's expression has the merit of greater accuracy and simplicity. We hope that engineers, who are apt to be somewhat loose in the naming of objects, will adopt Mr. Blechynden's terms. Added to the paper are tables giving details of construction and performance of representative steamers of the present day. A long discussion followed the reading of this paper. It turned chiefly upon the question of forced draught, corrugated flues, and the rules with regard to boiler testing which Mr. Sennett introduced when he was at the Admiralty. With regard to the forced draught question, the very sensible opinion seemed to have been arrived at that forced draught, though a good thing in itself, may prove a great ill if overdone. It is in the Navy chiefly that forced draught has gained an evil reputation, and naval officers are largely to blame for this, although the engineers must take their share of the responsibility. When it was found how great an accession of power could be obtained by forcing combustion with a fan, naval officers thought they had a royal road to speed. Boilers which had been designed on principles that had grown up under a simple chimney draught *régime*, were urged by fan-blast to duties beyond their powers of endurance; and then, when tube plates buckled and tubes leaked, forced draught was said by gallant admirals to be "the invention of the Evil One." The engineers, as we have said, were also to blame. The boiler has always been the Ishmael of the machinery-designer, nearly all the attention having been lavished on the engine. As a consequence boiler construction has been a matter of rule of thumb, and, when the empirical rules upon which it was based have no longer applied, the engineer has been nonplussed through want of a basis of scientific knowledge upon which to build anew. The torpedo-boat builders have no trouble with forced draught, though they blow far harder than in any other vessels; but then the torpedo-boat builders are good engineers—not mere blind followers of "practice"—as was proved by the paper read last spring on this subject by Mr. Yarrow before the Institution of Naval Architects. In speaking upon corrugated flues Mr. Macfarlane Gray made a remark on the subject which might have received more attention. It has long been claimed by the makers of this type of furnace that additional heating surface, and that of a most valuable kind, was obtained by the corrugations. This Mr. Gray said was a fallacy, for the heat from the furnace proceeded only in radial lines, and therefore no greater effective area of heating surface could be obtained than that due to a plain cylinder.

Mr. Shapton's paper was an interesting description of the building and machinery referred to in the title, by which grain is transported and stored. The warehouse in question consists chiefly of a vast cellular structure which might be described as a brick and mortar honeycomb, filled with grain in place of honey. There are 250 hexagonal bins or silos, each measuring 12 feet across the angles and 80 feet deep. The storage capacity is 2,240,000 bushels. The grain is lifted from vessels by elevators, and carried to the top of the building, from whence vertical movement is supplied by gravity. Horizontal travel is carried on by continuous moving belts or bands which run over wheel pulleys. The way in which streams of grain can be diverted into any required direction is very curious to watch. A good part of the discussion on the paper turned on the best form of bin or silo. At first one would think that the bin designer could not do better than follow the bee, but it was shown that cylindrical chambers made of sheet iron would give a large saving of space over the hexagonal brick bins. The advantage is due of course to the thinner walls of sheet iron, the cylinder being a form by which advantage can best be taken of the high tensile strength of iron. In America, where the silo system was in common use long before it made its appearance in this country, the bins are made wholly of wood, but this is subject to rot, and harbours weevils. Sheet-iron rusts and brick retains moisture, so that with brick the grain heats unless well looked after and ventilated. On the whole, however, brick has the preference in this country. Sir James Douglass made a suggestion which will, we should think, receive attention at the hands of future silo designers. The representation of the Eddystone Lighthouse at the Royal Naval Exhibition is a building not altogether dissimilar from a silo. It has very thin walls, which are constructed of expanded sheet steel, or sheared lattice work, which forms the bond for a crust of Portland cement. The result is a wall of great tenacity and rigidity, and one which would not have the same defect as brickwork with regard to harbouring damp. The problem of ventilating grain is

one of difficulty; and it may be said that it has not yet been solved. The most serious effort yet made was the building of a granary on the banks of the Thames, known, we believe, as the Patent Ventilating Granary. This granary was referred to during the discussion by Mr. Percy Westmacott, so long the chief of the hydraulic department at Armstrong's. The patent ventilating arrangement consisted of a perforated tube running down the centre of each bin. This was provided with a movable stop or plug, and, by adjusting the height of the stop, a blast of air could be directed through the perforations of the tube into any part of the grain. The idea was of French origin, and, Mr. Westmacott said, more ingenious than practical, so that the granary was pulled down after a time. It is easy to understand that those parts of the grain which required most ventilation would form into hard lumps, into which the air would not penetrate. As a matter of fact it is found more advantageous to air the grain by giving it a constitutional over the carrying bands.

Prof. Hele Shaw's paper on his experimental engine and alternative centre-testing machine was one of great interest. The engine in question, which is described as a marine engine, though it has a large fly-wheel, is, we believe, the most elaborate from an experimental point of view, yet made. The question has been raised whether it is not too elaborate, so that satisfactory results will not be reached on any one point. That is a problem which remains to be proved by facts; for the engine has only just been erected. It is 150 horse-power, and is of the ordinary vertical three-cylinder three-stage compound type. The high-pressure and intermediate cylinders have cylindrical valves, and the low pressure has a flat valve. Each valve is worked by a different type of motion—namely, ordinary Stephenson link motion, Joy gear, and Hackworth gear. The cylinders are jacketed at sides and ends, and there are provisions in the way of connections for working in every possible manner, *i.e.* cylinders all jacketed, not jacketed at all, or any one or two jacketed. Any combination of cylinders can be worked, or any one cylinder alone. In addition to this the cranks are adjustable on their shaft, so that any combination can be got in this way; in short, the number of different combinations that are at command would require years to work through. There are the usual measuring tanks and other apparatus for quantitative tests. An excellent suggestion was made by Prof. Goodman during the discussion. He proposed that arrangements should be made for testing the students' knowledge by putting the engine into conditions not in accordance with proper design. For instance, he would have valve-rods or excentric rods of improper length, valves ill-set with improper lap or lead, leaky valves and pistons, and various other ills, to which engines are subject, purposely introduced. He would also provide a means of passing water into the cylinders. He would then have the student take diagrams from the engine, and leave him to determine the cause of the defect by the appearance of the cards. We hope Prof. Goodman will be able to follow up this useful suggestion in his own laboratory at Leeds. The alternative testing machine is a 100-ton single-lever machine of the Wicksteed type. The alteration in power is got by substituting one fulcrum for another a few inches distant. The mechanism by which this is done is ingenious, but the details would be difficult to explain without the aid of diagrams.

The last paper read at the meeting was that of Mr. Leader Williams. The author commenced by saying that 46½ million cubic yards had to be excavated in making the Manchester Ship Canal, and as only 17,000 men and 200 horses have been used there was evidently required a large power in the shape of mechanical appliances in order to get the work done in anything like reasonable time. Ninety-seven steam excavators and eight steam dredgers of large power have been employed; and the spoil has in most cases been taken a distance of several miles. For this work, and for the general purposes of construction, 173 locomotives and 6300 trucks and waggons have been used. The railways laid for the purpose amount to 228 miles of single line. The rate of excavation has varied from three-quarters of a million to 1¼ million cubic yards per month. There are also employed on the works 124 steam-cranes, 192 portable and other steam-engines, and 212 steam-pumps. The coal consumed by the engines is about 10,000 tons a month. These figures will give some idea of the heroic proportions upon which large constructive works are carried out, and the capital required to start them. The whole plant of the Manchester Ship Canal has cost, we believe, close on a million sterling. The machines described in the paper which were of greatest interest were the excavators. The chief of these is the now well-known "steam navvy," made by Ruston and Proctor, of Lincoln. It first came prominently into notice during the construction of the Albert Docks, and is looked on as a standard tool wherever large excavating work is undertaken. It has the immense advantage of being able to work in any kind of soil, even including sandstone rock, if not very hard. It is only in hard rock that blasting has to be done as an auxiliary. The most interesting, or, perhaps, we should say the most novel machines are the French and German excavators, or land dredgers, which have been introduced into this country for the first time in connection with this work. These are on the same general principle as a floating ladder and bucket dredger of the common type. In place of the ladder and motive machinery being held by a floating hull, there is a small house mounted on wheels, and this runs on a line of rails on the summit of a bank. The ladder slopes outward from the side, reclining on the bank, which the buckets scrape away as they traverse, and deposit the spoil in waggons on the bank above. There are differences in detail between the French and German types, but in general principle they are alike. The German machine appears to us the better designed, but Mr. Leader Williams says the French excavator is of more substantial construction. The weight of these machines is from 70 to 80 tons, and under favourable conditions they have been known to excavate the enormous bulk of 2400 cubic yards in one working day. Mr. Williams's paper was not discussed, which is a fact to be regretted by engineers, as the subject is one which requires ventilation; but time was running short. After the usual votes of thanks, the sittings of the meeting were brought to a close.

We can only add a few words about the excursions. On the Tuesday there was a lunch on board the big White Star liner the *Majestic*, for one section of the members; whilst others visited the grain warehouse, described by Mr. Shapton in his paper, and the new overhead railway, which has been designed by Mr. Greathead, the Engineer of the City and South London Railway, and which runs along the line of docks. This railway is of steel and iron throughout, and possesses the novel advantage of forming a water-tight roof, under which the people of Liverpool will be able to walk on rainy days without getting wet. In the evening there was a *conversazione*, which, of course, was the social feature of the meeting. On Wednesday afternoon the members visited the new engineering laboratories which have been added to University College, Liverpool, where the engine and testing machine described in Prof. Hele-Shaw's paper were examined. On Thursday one party visited the Mersey Docks, the Mersey Tunnel, and Laird Bros.' ship-yard and engine works. At the latter there are several interesting vessels in progress, including the big battle ship *Royal Oak*, of 14,000 tons. Another party went to Horwich, and saw the fine locomotive works which have just been completed there by the Lancashire and Yorkshire Railway. These works have been beautifully planned and laid out under the superintendence of Mr. Aspinall. Although not so large as some other establishments of a similar kind, they may be taken as a model of design. Mr. Aspinall naturally had a unique opportunity with a clear field to work upon, and an accumulated experience at his command. Friday, the last day, was devoted wholly to the Manchester Ship Canal, the members being carried down the line of works in a special train, under the guidance of Mr. Leader Williams.

THE NEW GAS, CHLOROFLUORIDE OF PHOSPHORUS.

As briefly announced in the report of the proceedings of the French Academy of Sciences, a note upon a new gaseous compound, containing phosphorus, fluorine, and chlorine, has just been presented by M. Moissan, on behalf of M. Poulenc. During the course of his work upon the fluorides of phosphorus, M. Moissan observed that, when phosphorus trifluoride was brought in contact with chlorine, the green colour of the latter at once disappeared, and there appeared to be formed a new and colourless gas. The gas thus formed has been prepared in considerable quantity by M. Poulenc, and its properties investigated. It appears to be directly formed by addition, according to the simple equation—

$$PF_3 + Cl_2 = PF_3Cl_2;$$

for the trifluoride of phosphorus and chlorine are found to react in equal volumes, and the combination is attended by a contraction of one-half. The new gas may therefore be considered as phosphorus chlorofluoride, PCl_2F_3, the chlorine derivative of phosphoryl and thiophosphoryl fluoride, POF_3 and PSF_3.

The most convenient mode of preparation is described as follows. Two flasks of equal capacity (about 500 c.c.) are taken, and filled respectively with phosphorus trifluoride and chlorine. They are connected together by a bent tube passing through the stoppers, and the flask containing the phosphorus trifluoride is further connected with a reservoir of mercury in such a manner that a gentle pressure may be placed upon the trifluoride, so as to gradually displace it over into the chlorine. The two flasks being of equal capacity, it is evident that, when the whole of the trifluoride has thus been transferred, the reaction is completed, the green colour of the contents of the other flask disappears, and the remaining gas is almost pure chlorofluoride. After allowing to stand a few days in contact with the mercury, in order to remove the last traces of chlorine, the gas is ready for examination.

Phosphorus chlorofluoride is a colourless incombustible gas, possessing a powerfully irritating odour. It is instantly absorbed and decomposed by water and by solutions of alkaline or alkaline earthy hydrates. A determination of its vapour-density gave the number 5·40, sufficiently near the theoretical density of a substance PCl_2F_3 (5·46). It is comparatively easily liquefied, a temperature of $-8°$ C. being sufficient at ordinary pressures. It is dissociated at a temperature of 250° C. into gaseous pentafluoride and solid pentachloride of phosphorus. The induction spark effects the same decomposition.

Sulphur reacts with phosphorus chlorofluoride in a most interesting manner. The reaction commences about the melting-point of sulphur, 115° C., and the products are chloride of sulphur and gaseous thiophosphoryl fluoride, PSF_3. And here a most emphatic protest must be made against the manner in which many French chemists persistently ignore the work of the chemists of other countries. Thiophosphoryl fluoride, PSF_3, was discovered and prepared three years ago in the Research Laboratory of the Royal College of Science, South Kensington, by Prof. Thorpe and Mr. J. W. Rodger; and a detailed account, illustrated by experiments, of the mode of preparation and properties of this remarkable gas, was laid before the Chemical Society and published in their Journal.[1] And yet, in the memoir just presented by M. Moissan, we find this compound, a description of which long ago found its way into the abstracts or referate of most foreign journals, described as "un nouveau composé gaseux." Indeed, a considerable amount of unnecessary trouble appears to have been taken in order to ascertain the composition of this "new gas"—trouble which, as the compound is so readily recognizable by its extraordinary properties, might have been saved, if the author had taken the pains to look up the literature of the subject. It is high time that French chemists should look to their "prestige" in this respect, for, unfortunately, the present is by no means the only case which has within the last few months come before the notice of the writer of this note, in which compounds fully described and worked out by English chemists have been rediscovered and described as new by French authors.

When phosphorus chlorofluoride is passed over free phosphorus heated to 120°, it is decomposed with formation of phosphorus trifluoride, which passes away as gas, and phosphorus trichloride, which condenses in liquid drops. Metallic sodium, when slightly heated, appears to absorb the chlorofluoride entirely, while magnesium, aluminium, iron, nickel, lead, and tin, when heated to about 180°, attack the gas with formation of anhydrous chlorides and liberation of phosphorus trifluoride. Mercury attacks it very slowly at the ordinary temperature, but very rapidly at 180°, with formation likewise of a chloride of the metal and gaseous trifluoride of phosphorus. Hence, when purifying the gas from the last traces of chlorine, the mercury should not be agitated, but allowed to remain at rest, as agitation brings about a perceptible amount of decomposition.

Water reacts in two stages with phosphorus chlorofluoride. When a little aqueous vapour is admitted into the vessel inclosing the gas, phosphoryl fluoride and hydrochloric acid are formed in accordance with the equation—

$$PCl_2F_3 + H_2O = POF_3 + 2HCl.$$

When passed into water, however, the gas is completely decomposed into phosphoric, hydrochloric, and hydrofluoric acids—

$$PCl_2F_3 + 4H_2O = H_3PO_4 + 2HCl + 3HF.$$

Ammonia gas reacts at the ordinary temperature with production of a white solid compound, readily soluble in water, which appears to be fluophosphamide, $PF_3(NH_2)_2$.

$$PCl_2F_3 + 4NH_3 = PF_3(NH_2)_2 + 2NH_4Cl.$$

Phosphorus chlorofluoride is absorbed by absolute alcohol with production of a compound possessing a penetrating odour, and which burns with a bright flame bordered with green, and leaves a white residue of phosphoric acid. The nature of this compound has not yet been fully ascertained.

These properties of phosphorus chlorofluoride indicate that the gas is much less stable than the pentafluoride, and that the two atoms of chlorine possess a mobility which renders their removal a matter of considerable ease.

A. E. TUTTON.

[1] Journ. Chem. Soc. Trans, 1889, vol. lv. p. 306.

PROF. MENDELEEFF ON THE VARIATION OF THE DENSITY OF WATER AT DIFFERENT TEMPERATURES.

THE last number of the Journal of the Russian Physical and Chemical Society (1891, No. 5) contains an important paper, by Prof. Mendeleeff, upon the variation of the density of water at different temperatures. In a work, published in 1884 and translated into English in the Journal of the Chemical Society, the Russian Professor proposed the formula $S_t = S_0(1 - kt)$ as a first approximation to a mode of expressing the expansion of liquids at a certain distance from the temperatures at which they change their state, and within the limits of accuracy attained in the present determinations. But he remarked that the expansion of water would require a separate formula, and he now proposes the formula

$$S_t = 1 - \frac{(t-4)^2}{(A+t)(B-t)C},$$

which embodies, with sufficient accuracy, all that is yet known about changes in the density of water (S_t) within the limits of from $-10°$ to $+200°$. For all liquids save water, the increase of density with the increase of temperature, that is, the derived $\frac{ds}{dt}$, varies but little; it but slightly increases or slightly decreases with considerable changes of temperature; while for water, $\frac{ds}{dt}$ not only changes its sign at $+4°$, but very rapidly varies even at temperatures remote from zero, and even superior to 100°. After confirming the above by a few examples, Prof. Mendeleeff indicates the faint relations between his new formula for water and the general law of the expansion of liquids, by explaining the way in which he arrived at his new formula. He points out, moreover, that under the present state of the determinations of the density of water at various temperatures, it would be impossible to find exact figures for the constants A, B, and C, in the above formula, and that provisionally, and especially for temperatures between 0° and 40°, they may be taken as follows:—$A = 94·10$, $B = 703·51$, and $C = 1·90$.

Prof. Mendeleeff then goes on briefly to analyze the various corrections which ought to be taken into account in the determinations of the density of water; namely, the influence of pressure, the expansion of solids, and the measurements of temperature. All these being taken into account, it appears that the errors of the best determinations of densities attain several units in the fifth decimals, even at common temperatures. After many unsuccessful attempts at improving the current figures of densities by introducing into them several corrections, Prof. Mendeleeff abandoned the idea, and he now gives the authentic figures, as they were published by the investigators themselves, simply expressing all determinations in volumes for the sake of facilitating comparison. The figures published by Hallström (1823), Muncke (1828), Stampfer (1831), Despretz (1837), Pierre (1847), Kopp (1847), Plücker and Geissler (1852), Hagen (1855), Henrici (1864), Jolly (1864), Matthissen (1865), Weidner (1866), and Rosetti (1869), are thus given in a first table. The figures, as they were corrected by Biot in 1811, Hallström in 1835, Miller in 1856, Rosetti in 1871, Volkmann in 1881, Mendeleeff in 1884, and Makaroff in 1891, are given in a second table.

The averages of the volumes of water derived from the original

figures (Table I.), at temperatures from $-5°$ to $+100°$, taking the volume at $4°$ equal to 1,000,000, and the pressure being equal to one atmosphere, appear as follows in the second column (V_t) of the subjoined table. They are followed, in the third column, by the volumes as calculated from Prof. Mendeleeff's new formula :—

t.	V_t.	$V_t = \frac{1}{S_t}$ calculated from the formula.	$\frac{dV}{dt}$ for 1°.	$\frac{dV}{dp}$ for 1 atmosphere.	Possible errors of the present determinations.
−5°	1 000 662	1 000 676	−157	−52	∓ 29
0	1 000 122	1 000 127	− 65	−50	∓ 12
+5	1 000 008	1 000 008	+ 15	−48	± 3
10	1 000 263	1 000 262	+ 85	−47	± 15
15	1 000 847	1 000 849	+148	−46	± 26
20	1 001 733	1 001 731	+204	−45	± 35
25	1 002 871	1 002 880	+254	−44	± 43
30	1 004 248	1 004 276	+302	−43	± 49
40	1 007 700	1 007 725	+356	−41	± 59
50	1 011 933	1 011 967	+461	−40	± 67
60	1 016 915	1 016 926	+530	−39	± 75
70	1 022 513	1 022 549	+595	−40	± 85
80	1 028 849	1 028 811	+656	−41	± 98
90	1 035 719	1 035 692	+719	−42	±118
100	1 043 180	1 043 194	+781	−44	±145

Finally, a third table is given, being the result of the calculation made by taking

$$S = 1 - \frac{(t-4)^2}{1000\phi(t)},$$

$$\phi(t) = 128{\cdot}78 + 1{\cdot}158t - 0{\cdot}0019t^2,$$

and

$$1000\phi(t) = 1{\cdot}90\,(94{\cdot}10 + t)\,(703{\cdot}51 - t),$$

and extending the calculation to $+200°$ and $-10°$. The most important values of $\frac{ds}{dt}$ are given in the fourth column of the subjoined table ; so, also, the approximate values of $\frac{ds}{dp}$, which are "but a first rough approximation," to show the importance of pressure in the determinations of volumes of water :—

$t°$ C.	Calculated densities, S_t.	Possible error of present measurements (in 1,000,000th parts).	Derived ds/dt for 1° C. (in 1,000,000th parts).	Derived ds/dp for 1 atmosphere (in 1,000,000th parts).	Numerical values of $\phi(t)$.	Calculated V_t.
−10	0·998 281	∓ 49	+ 264	+54	114·01	1·001 722
5	999 325	∓ 29	+ 157	52	119·94	000 676
0	999 873	∓ 12	+ 65	50	125·78	000 127
+ 5	999 992	± 3	− 15	48	131·52	000 008
10	999 738	± 15	− 85	47	137·17	000 262
15	999 152	± 26	− 148	46	142·72	000 849
20	998 272	± 35	− 203	45	148·18	001 731
25	997 128	± 43	− 254	44	153·54	002 880
30	995 743	± 49	− 299	43	158·81	004 276
40	992 334	± 53	− 380	+41	169·06	1·007 725
50	988 174	65	− 450	40	178·93	011 967
60	983 356	72	− 512	39	188·42	016 926
70	977 948	80	− 569	39	197·53	022 549
80	971 996	92	− 621	40	206·26	028 811
90	965 537	109	− 670	41	214·61	035 692
100	958 595	133	− 718	42	222·58	043 194
120	943 314	±600	− 810	+43	237·38	1·060 093
140	926 211	650	− 901	48	250·66	079 667
160	907 263	700	− 995	55	262·42	102 216
180	886 393	750	−1093	64	272·66	128 167
200	863 473	800	−1200	73	281·38	158 114

In conclusion, Prof. Mendeleeff repeats that he proposes as soon as possible to make anew the determinations of the densities of water, because the former determinations were made on assumptions (permanency of the coefficient of the expansion of glass and mercury, and no notice being taken of pressure) which can no longer be maintained. If new measurements confirm the formula, or lead to a more correct one, we shall be better able to understand the laws of the expansion of all liquids, and therefore of gases as well. "In the case of water," he says, "we have begun to understand more clearly the influence of heat upon densities and volumes, and I believe that with the help of water we may expect some further progress in the study of the influence of heating upon matter."

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

OXFORD.—The examiners in the Final Classical Schools issued the Class List on Wednesday week, completing the results of the examinations held in Trinity Term.

The summer meeting of Extension students commenced on Friday last, when Mr. Frederic Harrison, M.A. Wadham College, delivered the inaugural lecture. The popularity of the movement is proved not only by the continual increase in the number of students who avail themselves of the advantages offered by this system of education, but by the interest which foreign Governments are taking in the development of the plan. The French Government have sent two special commissioners to report on the prospects and condition of the University Extension movement, and a large number of the representatives of the American University Extensionists are now in Oxford.

The number of students attending the various lectures is greater than on any previous occasion, more than 1100 having subscribed, while last year the number did not greatly exceed 900. A more rapid growth and a still greater measure of success attending the work may be anticipated from the fact that various County Councils, finding themselves in possession of funds arising from the operation of the Local Taxation Act, and which they propose to devote to the purposes of technical instruction, are availing themselves of the machinery of the University Extension system to accomplish this desirable end.

SCIENTIFIC SERIALS.

IN the *Botanical Gazette* for June, Mr. T. Holm contributes a study of some anatomical characters of North American grasses. In a paper entitled "On the Relation between Insects and the Forms and Characters of Flowers," Mr. T. Meehan epitomizes his views on fertilization opposed to the current theory, viz. that the part played by insects in the fertilization of flowers has been greatly exaggerated ; that flowers do not abhor cross-pollen ; and that all annuals can self-fertilize when cross-fertilization fails, annuals in almost all cases having every flower fertile.

THE most important paper in the *Journal of Botany* for July is the commencement of a detailed account of the Algæ of the Clyde sea-area, by Mr. George Murray, Secretary to the Committee for the Exploration of the Marine Flora of the West of Scotland. This is prefaced by an account of the physical features of the Clyde sea-area, by Dr. John Murray. Following this is the commencement of a hand-list of the Algæ, by Mr. E. A. L. Batters. The Rev. H. G. Jameson concludes his key to the genera and species of British mosses, which it is hoped may be published in a separate form ; and Mr. George Murray sinks Hooker's genus of sea-weeds *Cladothele* in *Stictyosiphon*.

SOCIETIES AND ACADEMIES.

PARIS.

Academy of Sciences, July 27.—M. Duchartre in the chair.—Proofs that Asia and America have been connected in recent times, by M. Emile Blanchard. In this paper the author points out certain species of Asiatic fauna and flora which are found in North America, as, in the preceding one, he indicated the representatives of European fauna and flora which occur in the same continent. Without making an extensive enumeration of the

different species, the facts brought forward give considerable support to the idea that Europe, Asia, and America have been connected by land in comparatively recent times.—The Ichthyosaurus from St. Columbe, by M. Albert Gaudry. This is a description of an Ichthyosaurus exhibited at the Paris Exhibition of 1889. It is proposed to name the fossil *Ichthyosaurus burgundiæ*.—Examination of samples of native iron of terrestrial origin discovered in gold washings from the environs of Berezowsk, by MM. Daubrée and Stanislas Meunier. The specimens examined weighed respectively 11·5 grams and 72 grams, and were discovered near the Berezowsk gold mines, Persia. The metal is very magnetic, but manifests no polarity. Its density is 7·59. When treated with an acid it is sensibly attacked, but does not show the Widmanstätten figures as is the case when acid is applied to a clean face of meteoritic iron. This fact and the absence of nickel leads the authors to conclude that the iron is truly native. About one per cent. of platinum is present.—On the volatility of nickel under the influence of hydrochloric acid, by M. P. Schützenberger. When dry hydrogen is passed over pure anhydrous nickel chloride at a red heat, it may be shown that the hydrochloric acid gas which comes off from the tube in which the reduction occurs contains a sensible amount of metal in the form of a volatile product. The same result is obtained if, instead of reducing nickel chloride by hydrogen, finely divided nickel is acted on by dry hydrochloric acid gas. M. Schützenberger has not yet been able to isolate this body for the purpose of determining its constitution. —Note on a proposed Observatory on Mont Blanc, by M. J. Janssen.—On the retardation of luminous impressions, by M. Mascart.—Works of applied zoology effected at the Endoume maritime station during 1890, by M. A. F. Marion.—On a geometrical representation and formula expressing the law of the passage of perfect gases through orifices, by M. Henri Parenty.—On the densities of oxygen, hydrogen, and nitrogen, by M. A. Leduc. The values obtained are: hydrogen 0·0695, oxygen 0·1050, nitrogen 4·9720. From the densities of oxygen and nitrogen the percentage proportion of the former element in air is found to be 23·235 by weight and 21·026 by volume. The atomic weight of nitrogen deduced from these results is 13·99, and that of oxygen 15·905.—Remarks on the transport of metallic iron and nickel by carbon monoxide, by M. Jules Garnier. Some observations of the character of the flames issuing from furnaces in which these metals are being reduced are shown to be easily explained in the light of recent work on iron and nickel carbonyls.—Action of water on the basic salts of copper, by MM. G. Rousseau and G. Tite. Certain borates and oxychloride of antimony are transformed to oxides by the prolonged action of water at a sufficiently high temperature. Similarly, by heating copper nitrate, brochantite, and atacamite with water in sealed tubes they have been reduced to oxides. Libethenite has been experimented upon, but has resisted the transformation even when kept in the presence of water for three days at a temperature of 273° C.—On an actual mode of formation of mineral sulphides, by M. E. Chuard.—Researches on thallium, by MM. C. Lepierre and M. Lachaud. Thallium chromate has been prepared by acting on thallium sulphate with potassium chromate. Reactions with various bodies are described.—On parabanic and oxaluric acids, by M. W. C. Matignon. The heat of combustion of parabanic acid is found to be 212·7 cal., of oxaluric acid 211 cal. Hence the heats of formation have been calculated, 139·2 cal. and 209·9 cal. The heat of solution of parabanic acid at 20° and with a concentration of $\frac{1}{20}$ mol. per litre is − 5·1 cal. The formation from oxalic acid of its ureide, parabanic acid, gives + 2·2 cal. and of its uramic acid, oxaluric acid, + 2·5 cal. The formation of the ureides thus gives only a feeble heat-liberation. Each of these acids dissolved in a large excess of potash yields the neutral potassium oxalate. Potassium oxalurate has been prepared by dissolving the acid in its equivalent of potash and evaporating. Fine prismatic needles are obtained, differing from the salts of Menschutkin and Strecker. The heat of neutralization of oxaluric acid is 30·2 cal., as against 34·2 cal. for oxalic acid.—The transformation of gallic acid and tannin into benzoic acid, by M. Ch.-Er. Guignet.—On the polymeric acids of ricinoleic acid, by M. Scheurer-Kestner.—On the fermentation of bread, by M. Léon Boutroux. During an examination of the conditions essential for the fermentation of bread, the author has isolated five species of yeast and three species of bacteria. The parts played by each of these organisms are described, and the conclusion is finally drawn that the fermentation of bread consists essentially of a normal alcoholic fermentation of sugar pre-existing in the flour, and that only the yeasts producing alcoholic fermentations are necessary; the ordinarily co-existing alteration of gluten is a subsidiary and unessential action due to some of the bacteria present.—On a thermogenic substance in urine, by M. Paul Binet.—On the transformation of carboxy-hæmoglobin into methæmoglobin, and a new process of examination for carbon monoxide in the blood, by MM. H. Bertin-Sans and J. Moitessier.—On a new apparatus for measuring muscular power, by M. N. Gréhant.—Measure of the muscular power of animals under the action of certain poisons, by MM. Gréhant and C. Quinquaud.—On the concordance of Prof. S. P. Langley's experimental results on the resistance of the air (see NATURE of July 23, p. 277) with the values obtained by calculation, by M. Drzewiecki.—Analysis by means of chrono-photography of the movements of the lips during speech, by M. G. Demeny. Using M. Marey's method for photographing objects in rapid motion, the author has succeeded in portraying the movements of the lips during speech, and finds that it is possible to distinguish the letters of the alphabet when the photographic results are spun in a zootrope.—Relation between oscillations of the retina and certain entoptic phenomena, by M. A. Charpentier.—The nanny-goat is not refractory to tuberculosis, by M. G. Colin.—Researches on the pathogenic microbes in muds from the Dead Sea, by M. L. Lortet.—On the excretory apparatus of Carididæ, and on the renal secretion of Crustaceæ, by M. P. Marchal.—On the nervous system of Monocotylidæ, by M. G. Saint-Remy.—Contribution to the natural history of a cochineal, *Rhizœcus falcifer*, Künck, discovered in the greenhouses of the Museum and living on the roots of the vine in Algeria, by MM. Kunckel d'Herculais and Frédéric Saliba.—On specific assimilation in Umbelliferæ, by M. Génean de Lamarlière.—Document relative to the trajectory of the Ensishein meteorite of 1492, by Prof. H. A. Newton.—On the erosion and transport by torrential rivers having glacier affluents, by MM. L. Duparc and B. Baeff.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

Cosmical Evolution: E. McLennan (Chicago, Donohue).—The Artillery of the Future and the New Powders: J. A. Longridge (Spon).—British Rainfall, 1890: G. J. Symons and H. S. Wallis (Stanford).—Epidemic Influenza, Notes on its Origin and Method of Spread: Dr. R. Sisley (Longmans).—Essays upon Heredity and Kindred Biological Problems; authorized translation, vol. i., 2nd edition: Dr. A. Weismann, edited by E. B. Poulton, S. Schönland, and A. E. Shipley (Oxford, Clarendon Press).

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